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CONDUCTED BY

G. M. HUMPHRY, M.D. F.R.S.
PROFESSOR OF ANATOMY IN THE UNIVERSITY OF CAMBRIDGE,
HONORARY FELLOW OF DOWNING COLLEGE;

AND

WM. TURNER, M.B.
PROFESSOR OF ANATOMY IN THE UNIVERSITY OF EDINBURGH.

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ON THE CAUSES OF THE SECONDARY WAVES SEEN IN THE SPHYGMOGRAPHIC TRACING OF THE PULSE. By A. L. Galabin, M.A., M.D., *Fellow of Trinity College, Cambridge.*

It is a fortunate circumstance for the application of the Sphygmograph, as a means of clinical research, that a knowledge of the cause of the several secondary waves of the pulse is not necessary for their practical interpretation. This may be learnt empirically by watching the association of the different forms of pulse-curve with the known conditions of the patients from whom they are obtained. Thus we find that authors who differ totally from each other as to the causation of any particular form of pulse, and even as to the state of circulation which it implies, yet are quite in agreement as to its clinical import, and the prognosis to be derived from it. But it requires much experience of tracings to be able to draw these inferences, and those who have not this are apt to interpret what they see from theory, and thereby easily fall into error. For instance, it has often been supposed that a high sharp primary summit, followed by a sudden fall, is a sign of aortic regurgitation, but this may occur just as much in the pulse of simple excitement. Hence, from the practical point of view, the study of causes is useful, and it is essential for the arriving at general physiological conclusions, such as the determining what is the true state of the circulation in fever, or in Bright's disease, or what is the effect on the vascular system of various drugs.

It needs but little study of the literature of the subject to discover that scarcely any two authorities agree together

as to the cause of the several waves, and of their variation. The question is one which must be settled by having regard both to the principles of mechanics, and to the results of observation. And it is impossible to avoid suspecting that some of those who have treated the subject experimentally, would have been assisted in the interpretation of their results, if they had possessed some theoretical knowledge of Hydrodynamics.

I have adopted an experimental mode of investigation by the use of a combination of bifurcating elastic tubes to represent the arterial system. I have not attempted to imitate the individual arteries of the body, for although such an apparatus looks well in a lecture-room, it does not any the more resemble the actual complexity of the human circulation. To these tubes I have adapted, in some cases, the heart of a sheep, in others, an artificial heart of india-rubber, and the contraction of the heart has been imitated by manual compression. It will be seen that counterparts have thus been obtained of the most important types of pulse found in the human body. Tracings 1 to 4 were procured with the real heart, those from 5 to 9 with the artificial. In the case of the real heart, I have found it more convenient to use the right side, for the thick walls of the left ventricle form an obstacle to manual compression. The aortic, or pulmonary valves act efficiently after death, but it is not so with the mitral or tricuspid. It follows from this, that the real heart will only work against a low pressure, and the highest which can be obtained with it (fig. 3) corresponds to nearly the lowest ever found in the living body, while the tracings at lower pressures, such as fig. 1, represent a state of things which never occurs in arteries. Tracings from the real heart, at the higher pressure, resemble closely those obtained at a similar pressure from the artifical heart, thus showing that the action of the latter is sufficiently like that of the real heart for the purpose of these experiments (compare tracings 2 and 5). It has been considered by Dr Burdon Sanderson impossible to make the contraction of the hand sudden enough to imitate that of the living heart, but I have not found this difficulty at all insuperable.

I had, in the first place, made use of a single elastic tube, corresponding to the schema constructed by Dr Burdon Sanderson. In this case I found the tracings different at different parts of the tube, and the variation was such as to show the presence of retrograde waves due to reflection from the end of the tube. This reflection took place as much from an open as from a constricted orifice. In my final combination, I made the several tubes of very unequal lengths, and in using compression in order to vary the tension, I applied it to each of the smallest tubes, not at one, but at several points, so that no reflected waves might be called forth of a kind which could not occur in the body. Applying then the same test of taking tracings at different distances, their resemblance to each other showed, as it does in the case of the arteries of the body, that all the waves were direct and not retrograde. The effect of friction in the capillary circulation was thus imitated by that of compression at several points of each small tube, and if it may be said that reflection might still possibly take place in the schema from the latter, so it has been held by many that it occurs in the body from the former. I shall refer to the several tracings obtained under different conditions as the points arise which they illustrate.

It was shown so long ago as in 1833 by Weber, that the motion of the pulse is that of a wave. By this term is meant the transmission with a definite velocity, not of matter, but of a state of motion and of pressure. It follows from the theory of waves, that on the large wave other waves may be superposed, which run each their own course almost exactly as if they existed alone, and which may be added together to form compound waves.

Among those who have not given special attention to the Sphygmograph, almost exclusive regard has been paid to one of the secondary waves of the pulse, namely, the dicrotic wave, and no doubt this is, to a great extent, justified by its importance in the theory of the pulse, its constant occurrence, and its great development in fever. But there is another wave preceding it, which sphygmographers find to be of quite as great clinical significance, and which varies as widely, for, while often absent, it may attain considerable dimensions. Being but

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slightly marked in a healthy pulse (fig. 10), and often disappearing in that of a student (fig. 19), it forms a stronger feature in the pulse of an athlete, and shews itself also in that of ladies under the influence of excitement. Under another form again, with advancing years, it acquires a magnitude which is of ominous significance, but gains perhaps its greatest development in cases of chronic Bright's disease (fig. 12). I believe this wave would be better known if it had a convenient name. "First secondary wave" is too long, so is "systolic pressure-wave," and, moreover, that asserts a theory. The only short name applied to it is that of "tidal-wave," used by Mr Mahomed in his papers in the *Medical Times.* Although this also is intended to imply a theory, which I believe to be erroneous, yet, as it does not assert it so directly, I shall adopt it for its convenience. I propose, therefore, to discuss first the tidal, and afterwards the dicrotic wave, and in connection with the former it is necessary to consider the primary upstroke.

I will notice first existing theories. Dr Burdon Sanderson, writing in 1866, says that the contraction of the heart produces two waves, one of accelerated movement, and one of increased tension; that these, starting together, become separated in the distant arteries, because the former travels with a velocity of about 90 feet per second, and so is practically instantaneous. He compares the former to the communication of a blow along a line of ivory balls, but afterwards, accepting a correction on the ground that water is not elastic, admits that, but for the effect of the elastic receptacle, the resemblance would be rather to balls of clay. Now in the popular sense of the word elastic, that of compressible, water is almost inelastic, but in the more exact sense, in which elasticity is measured by the perfection of recoil to any force impressed, water is very elastic, as is shown by the fact that sound is conveyed much better in water than even in air. So far it is more like ivory than clay, but in truth the behaviour of neither has the slightest analogy to that of a fluid. It is the first principle of the mechanics of fluids, that at every point of a fluid, whether at rest or in motion, pressure is the same in all directions. It follows from this that there can be no wave of forward pressure (and without that there can be no accelera-

tion), which is not at the same time a wave of lateral pressure, expanding the walls of the tube.

The view of Dr Burdon Sanderson is therefore mechanically impossible: it is also contrary to observation. *In the first place,* the distinction of the two waves should, according to him, be most when tension is lowest. But, on the contrary, the tidal wave is most separated in pulses of high pressure, like that of Bright's disease, altogether absent in the febrile pulse of low tension, and precisely the same relation was found in my experiments with tubes. *Secondly,* the tidal wave should be farther apart from the primary wave in the more distant arteries; but a comparison of tracings 15, 17, and 18 from the brachial, radial, and dorsalis pedis, shows that this is not the case. And *thirdly,* it is not the fact that the wave producing the first upstroke is practically instantaneous. In the case of elastic tubing the interval is very perceptible, if a length of 6 or 8 feet be taken. The velocity varies altogether according to the diameter of the tube, its material, and tension, but in one case I found it to be less than 20 feet per second. The same thing has been shown as regards the body, in this *Journal,* by Mr Garrod, who by his Cardio-Sphygmograph measured the interval between the heart's contraction and the first upstroke of the pulse tracing. Weber, again, reckons the average velocity of the pulse wave to be about 28 feet per second.

Dr Anstie, writing in conjunction with Dr Burdon Sanderson, yet differs somewhat, at least in expression, for he speaks of the tidal wave as an expansive wave, which is a movement in the arterial wall, and so slower in propagation. This would seem to be the same as the theory adopted by Volkman to account, not for the tidal, but for the dicrotic wave, that one wave is transmitted in the fluid, another in the arterial wall.

Dr Balthazar Foster, as represented by the article in the last edition of Dr Aitken's *Medicine,* holds that the primary wave is a vibration of the blood column, travelling instantaneously, and that the next is a wave of distension by blood. It is obvious that a wave of vibration is quite a different thing from a wave of forward motion, yet Dr Burdon Sanderson would seem to have combined this view with his own, for he says in 1871, "The bursting open of the aortic valves produces a vibra-

tory movement of the blood transmitted instantaneously (that is, in about $\frac{1}{50}$ second)." Now one vibratory wave would indeed be transmitted, namely, that of sound, with a velocity due to the compressibility of water, not of 90, but of about 5000 feet per second, but this produces no motion in the lever. Tracing 22 was obtained by giving blows to a rigid part of the tubing. It shows that even a coarse and violent vibration produces hardly any upstroke. Dr Burdon Sanderson has published some tracings of waves produced by percussion, but in that case the blows were applied to the elastic tube, which would of course give rise, not only to vibration, but to waves of forward motion and expansion. Most of the other objections to the views of Dr Burdon Sanderson will apply also to those of Dr Foster.

Yet another theory of the tidal wave is maintained by Mr Garrod. Discarding the notion of an instantaneous wave due to the closure of the mitral valve, or the first impulse of the heart, he yet holds the tidal wave to be an instantaneous wave due to the closure of the aortic valves. I think this theory will commend itself to no one who has watched the variation of the tidal wave in many tracings; but, if any refutation be necessary, I would refer to tracing 8, showing its prolongation by a protracted contraction of the heart.

The view of Mr Mahomed, as far as I understand it, is the same as that of Dr Foster, except that he does not consider the first wave to be instantaneous.

Proceeding next to my own explanation, I have to remark first, that, since the sphygmograph is possessed of inertia, and is itself subject to the laws of motion, its construction must necessarily have some influence, however small, on the tracing produced. In sphygmographs having a secondary spring to depress the long lever, the tidal wave is often broken into two waves (fig. 21), and if this spring has a short time of vibration, a jagged line may appear. In the instrument I have used, of the form devised by Mr Mahomed, this spring is, with advantage, omitted, and no such waves are then ever seen. Thus from many tracings published, an experienced person may draw inferences, not only about the patient, but about the form of sphygmograph used. In order, therefore, to determine how

much is due to the instrument, since inertia cannot be got rid of, I have adopted, what is called by Mill, the "Method of Concomitant Variations," altering the moment of inertia of the lever about its axis of motion by a small sliding weight. The results are tracings 15 and 16. It will be seen that the relation of the primary and tidal waves is altogether altered, while the position of the dicrotic wave remains unchanged, although its amplitude is diminished. The method of measuring the position of these waves is first to draw a horizontal line of reference, then to place the tracing again on the moving plate of the sphygmograph, and draw curved lines with the writing point of the lever. It is obvious that if the tidal wave were a wave passing in the artery, its relative position could not be altered by weighting the lever of the sphygmograph; or, at any rate, if any effect at all were so produced, it could only be an apparent retardation, and not an acceleration of the wave. The separation of the primary and tidal waves is therefore due to an oscillation in the sphygmograph, caused by the inertia of the instrument, and the relation of the tracing to the true pulse wave is something like what I have drawn in fig. 23. In some cases the lever may be separated slightly from the knife-edge on which it rests, but generally the oscillation takes place in the instrument as a whole, and it may be followed by others in a descending series. Thus if the lever be moved, not by a knife edge, but by a rack and pinion adjustment, the tidal wave still occurs. Such an arrangement probably makes the tracing resemble the true pulse wave a little more closely, but for clinical purposes it is not so good.

I may mention that in the pulse shewn in fig. 15, in which the tidal and dicrotic waves are both so marked, one secondary beat could be felt by the finger, but only one. Such pulses do indeed give at first to the finger the impression of several secondary waves on account of their thrilling quality. But the sphygmograph gives no record of a thrill, as is shown by its application to the heart in the case of mitral stenosis. If the stenosis be moderate, the prolonged auricular contraction is shown in the tracing; but if it be so close that a thrill is continued throughout the whole period of rest, no record of it any longer appears.

My general conclusion is confirmed by the application of the little instrument which has been called a sphygmoscope, whereby the motion of the pulse is displayed by the variation of a jet of gas. By this means the dicrotic wave may readily be seen, but not a single tidal wave. Its place, however, is supplied by a slight quivering motion, which is due to the vibration of the elastic diaphragm, upon which the pressure of the pulse is received. This vibration varies with the size and tension of the diaphragm, and it might be possible so to adjust these that a wave should appear like the tidal wave of the sphygmographic tracing.

Another argument may be drawn from the fact that the development of the tidal wave varies in some degree according to the pressure which is applied to the artery. Thus in the pulses in which, at ordinary pressures, no tidal wave can be seen, it may sometimes be made to appear by using an excessively low pressure.

The explanation which I have applied to the whole tidal wave is adopted by Dr Burdon Sanderson, to account for the first of the two waves into which it is broken by the use of the secondary spring. But this must be entirely due to that spring, since it never occurs in its absence.

The view of M. Marey is, up to a certain point, the same which I have taken, for he says that the first pointed summit is due to the acquired velocity of the long lever. But he regards the tidal wave in some of its forms as an instantaneous wave due to the closure of the aortic valves: the dicrotic wave he believes to have no connection with those valves. We have seen that Mr Garrod has adopted the same theory as far as concerns the tidal wave.

Of all the diagrams in the work of M. Marey, one of the most interesting is that in which three simultaneous tracings are shown, of which the first represents the pressure within the right auricle, the second the pressure within the right ventricle, while the third is the tracing of the apex beat, obtained by means of an ampulla inserted within the thoracic wall. In all of these the primary summit is followed by two or more secondary eminences, much resembling the small waves which may be seen in the place of the tidal wave in the pulse tracing

as drawn by M. Marey's sphygmograph (Vid. fig. 21). I believe that their origin is similar, and that they illustrate the mode in which eminences may be produced in consequence of the inertia of the instrument. By M. Marey himself, however, they are attributed, in the case of the first two tracings, to an oscillation in the tension of the auriculo-ventricular valves, occurring after their closure, and causing a corresponding rise and fall of pressure within the two cavities. Now it is evident that if the oscillation of the valves causes a rise of pressure in the ventricle, it must, at the same moment, cause a fall of pressure in the auricle, and conversely. Hence the elevations in the tracing of the one cavity, if due to this cause, ought to be synchronous with the depressions in the tracing of the other. But on referring to the diagram of M. Marey, it will be found that the elevations in the auricular curve correspond, not to depressions, but to elevations in the ventricular curve, and therefore the explanation given by him would seem to be inapplicable.

While I thus believe that waves occur in the tracing, which have no separate existence in the pulse, I am yet of opinion that the instrument is more clinically useful than if it followed the artery more closely, for I think that slight differences in the form of the pulse wave, and in the suddenness of its commencement, are thus translated into a form much more manifest to the eye. The constancy of the form of the pulse tracing in the same person under similar circumstances, proves that it contains no casual oscillations, but that its form has, at least, a fixed and definite relation to that of the true pulse wave. My experiments with elastic tubes showed that the tidal wave could not be produced unless the pressure exceeded a certain point, and also the length of elastic tube were limited, or rigid tubing substituted for a part of it (fig. 7), and it could then be prolonged to a great extent by increasing the length of the heart's contraction (fig. 8). Its development thus indicates three things, high tension, diminished elasticity, and long laborious action of the heart. This conclusion agrees entirely with clinical experience of the kinds of pulse in which it is most manifested.

Passing on next to the dicrotic wave, it may be thought almost superfluous to consider its cause, since the common view,

ascribing it to a recoil produced by the closure of the aortic valves, appears so probable and intelligible. Yet the case is not so simple as it seems. Thus we find that Dr Burdon Sanderson, who, in 1866, appeared to adopt the common view, says in 1871, "The dicrotic wave has nothing whatever to do either with the closure of the aortic valves, or the cessation of the heart's contraction." His present theory is a little difficult of comprehension. He says, "In the largest arteries the expansion is ebbing, while in the smallest it is still culminating: so that for an instant the pressure is greater in the latter than in the former. The restoration of the equilibrium must take place by increase of pressure towards the heart, and diminution towards the periphery. This restoration of equilibrium constitutes the second beat." In another place he says that owing to the cessation of the heart's contraction the capillary arteries become relaxed, the capillary circulation retarded, and the aorta simultaneously distended in consequence of the increased resistance in front: and that this distension is in its turn propagated towards the periphery. These two accounts seem to me different, nor can I clearly gather whether or not Dr Burdon Sanderson considers the dicrotic wave to be retrograde. But such a transmission of influence from the periphery to the centre could only take place as a retrograde wave. To determine therefore whether this occurs I have compared two tracings from the same dicrotic pulse, one from the femoral artery just below Poupart's ligament, the other from the posterior tibial below the ankle. These are 19 and 20. The corresponding position of the dicrotic wave in the two shows that it is not retrograde but direct, and there is no retrograde wave at all present, for such a wave would be close to the primary wave in the posterior tibial, and farther from it in the femoral. The same tracings refute the theory that the dicrotic wave is due to reflection at the bifurcation of the aorta, for then it would be absent below that point. While I have thus opposed most of the views of Dr Burdon Sanderson, I should be the last to undervalue the service which he has done for the practical application of the sphygmograph, for I agree with his clinical inferences as completely as I differ from his mechanical explanations.

Perhaps the oldest theory of the dicrotic wave is that of

Dr Barlow, who observed it before the invention of the sphygmograph, and held it to be a reflection from the periphery. He believed dicrotism to indicate an obstructed capillary circulation, and therefore high tension, and to imply a stage of irritation and contraction, which, in disease, preceded the stage of fever and relaxation. The sphygmograph, however, has shown that great dicrotism belongs especially to the state of fever itself, and is found neither in the preceding stage, nor in that of exhaustion which follows. M. Marey and Dr Carpenter likewise consider the dicrotic wave to be a reflection from the periphery. Now this reflection is exactly what occurs in a single elastic tube, but not in the body, as already shown by the comparison of tracings 19 and 20. Duchek believes the dicrotic wave to be an oscillation, not in the aorta, but in the peripheral arteries. Vivenot regards it as an oscillation, but does not explain how it arises. The view of Volkman has been already mentioned.

Now, looking at tracing 1, from the elastic tube, and 14 from the radial pulse, it would seem that a dicrotic wave, equal in magnitude to the primary, could hardly be due to the simple closing of a valve. Experiments with tubes show that the dicrotic wave is only the first of a series, and when pressure is very low the aortic valves, being in glass tubes, can be seen to open and close a second time, after their first closure. In that case therefore the closing of the valves is not so much the cause as the effect of the secondary waves. I have tried the effect, in the case of the real heart, of dividing one of the semilunar valves, in that of the artificial heart of removing them altogether. The results are shown in fig. 4 and fig. 9. In both the dicrotie wave is less than before the alteration, but still very considerable. This fact of the occurrence of the dicrotic wave without any aortic valves has also been noted by Duchek, by Vivenot, and by Dr Burdon Sanderson. It agrees entirely with experience of tracings in the case of aortic regurgitation, for although the dicrotic wave is diminished when regurgitation is free, it is yet never absent, and even in a splashing pulse often retains considerable size. The use of the sphygmograph is thus disappointing for the diagnosis of aortic disease, although, the fact of regurgitation once known, it is of service in determining its extent.

My own view is, that, as the tidal wave is due to the inertia of the sphygmograph, so a wave occurs which is due to the inertia of the arterial walls. If it were not for this inertia, their distension would always be such as to be in equilibrium with the pressure of the fluid within at every moment. As it is, it takes a little time to reach this point of equilibrium, then by acquired velocity is carried a little beyond it, and so again passes within it as it recoils, and thus makes a series of oscillations about the equilibrium point. Thus there occur oscillations of expansion and contraction of the largest arteries, due to the effect of the inertia of the arterial walls on their lateral motion, but modified also by the inertia of the fluid. The first of these, the only one which commonly occurs, forms a part of the dicrotic wave. Be it especially observed that I attribute the oscillation to the inertia of the arterial wall and not to its elasticity, although that elasticity is of course necessary for this, as for every other part of the motion of the pulse wave, and its degree affects the period and extent of the oscillation. Nor, again, would any oscillation occur from the inertia of the fluid alone, so far as that affects its forward or backward motion; but such an oscillation being once set up, it is more ample the greater the momentum of the fluid, because the motion of the tube and of the contained fluid can only take place as a whole. It is thus that dicrotism is increased, as shown by M. Marey, if a denser fluid, as mercury, be taken instead of water. There is however another way also in which the inertia of the fluid may come into play, and that is by its effect on that slight lateral motion of the particles which must take place in consequence of the expansion of the tube. The effect of the acquired lateral velocity of such particles would be to expand the tube a little beyond the point which it would otherwise have reached, and by that means set up an oscillation. The effect of such acquired lateral velocity is generally disregarded in mathematical investigations of similar waves, as being too minute to have any appreciable effect. It would be difficult to ascertain whether in this case the part it plays in the general result ought to be taken into account. The aortic valves would produce a wave of their own without any oscillatory wave, and they also reinforce that wave by reflection. The second oscil-

latory wave I have found in but few pulses, of which one is shown in tracing 14. That pulse was in the highest degree compressible, and was taken only a few hours before death. The condition was just the opposite of that of the common tricrotic pulse (vid. figs. 11 and 15), in which the second wave is the tidal wave.

I think an argument for my view may be drawn from the possibility of the occurrence of a monocrotic pulse. According to the common theory of the dicrotic wave, this would imply that the aortic valves never close at all, in which case we can hardly suppose that the circulation could continue; but upon the other view, it only means that the rate of the pulse is equal to the rate of oscillation in the aorta. The dicrotic wave seen in a tracing may thus be made up of three waves superposed, the recoil from the aortic valves, the first oscillatory wave of the large arteries, and the second (or sometimes the first) oscillatory wave of the sphygmograph.

An opinion has been expressed by one of the ablest mathematicians of the day, especially in relation to physical problems, I mean Professor Maxwell, that no mathematical solution could be usefully applied to the theory of the pulse, and that for two reasons,—because the blood is not truly fluid, and because the motions of the arteries would be so much affected by their external attachments. These difficulties however would not apply to experimental elastic tubes, and since the principal forms of pulse can be imitated in them, I am led to the conclusion that blood is really fluid while contained in the arteries, and that their external connections are too loose greatly to modify their motion.

The introduction of the inertia of the arterial wall makes the question very complex for mathematical treatment. Disregarding that, a differential equation may be obtained of a form similar to that occurring in other kinds of wave motion. Its solution gives a velocity for the wave, which involves the material and diameter of the tube, and the pressure of the fluid, being greater when pressure is greater. This last result agrees with observation as to the retardation of the pulse, and is a much likelier explanation of that phenomenon than to suppose that what is felt by the finger is in some cases not the primary

upstroke, but the tidal wave, which is only a convexity in the descending curve. I think it will be found that the retardation is most in dicrotic pulses, where tension is low, and therefore the wave velocity less, but in which the tidal wave is entirely absent. Calculation gives no indication of the existence of any other wave travelling with a different velocity, except the wave of sound, whose velocity is due to the compressibility of water, and is nearly 5000 feet per second.

As a rule my experiments showed the dicrotic wave to be increased by diminution of pressure. This is the general view, and has been denied only by Mr Mahomed, on experimental grounds. But his schema differed from the arterial system by the introduction of a spherical elastic bag, which could hardly fail to introduce a set of oscillations of its own. It need hardly be said that it would be mechanically most unlikely, as well as for the sake of inference most unfortunate, that the same dicrotism should at different times employ opposite conditions. There is however one limitation to be made. If tension be increased by compressing a tube at a single point, dicrotism is often not diminished but increased, because the oscillatory wave is then kept in and reflected. This explains why dicrotism is increased by placing a tourniquet upon the abdominal aorta.

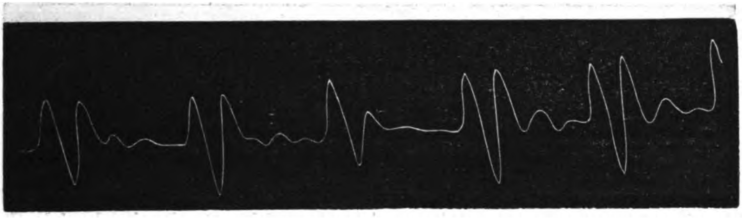
Theoretically both components of the dicrotic wave should be increased as pressure is lowered. The recoil from the aortic valves is not indeed greater, and could never produce such a dicrotic wave as that in fig. 1 and fig. 14, but it becomes more marked because preceded by a greater reflux, and consequent fall of pressure, when the valves close slowly. Oscillatory waves again are always more ample when tension is low.

In my experiments I have found that besides the variation of pressure, one other condition increases dicrotism, provided pressure be also low, namely to make the action of the heart short and sudden. In that case, however, the tracing has a rather different aspect, for sharper points are seen in the curve. This agrees with observations on the human pulse, for in that the rounding off of points is well known to be of bad prognosis. If the action of the heart be jerky, but at the same time pressure not low, the result is the common tricrotic pulse, in which the second wave is the tidal wave, and the first sum-

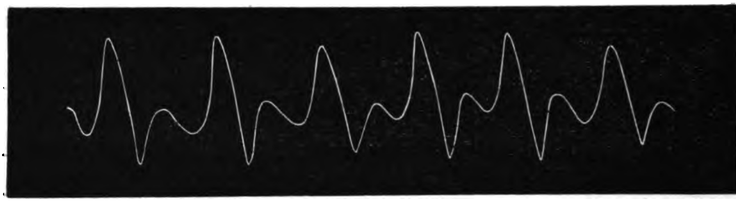
mit high and sharp. This state of things occurs in the body in the case of muscular exertion, mental excitement, from the smoking of tobacco, and, with a greater proportionate development of the tidal wave, in acute nephritis.

As an application of the foregoing principles, certain conclusions may be drawn as to the state of the circulation in fever. The degree of dicrotism together with the rounding off of sharp points indicates that arterial pressure is very low, and at the same time the action of the heart short and sudden, but that the former of these conditions preponderates. This would be explained by supposing a paralysis to occur of the nerves which cause contraction of the arterial walls. The rapidity of the heart would then be in part the direct consequence of low arterial pressure, according to the relation demonstrated by M. Marey, but its short and sudden action appears to indicate a disturbance of its own innervation in addition. The increased rate of circulation would however depend less on the action of the heart, than upon the arterial relaxation, which is therefore the most important element in the state of the vascular system existing in fever.

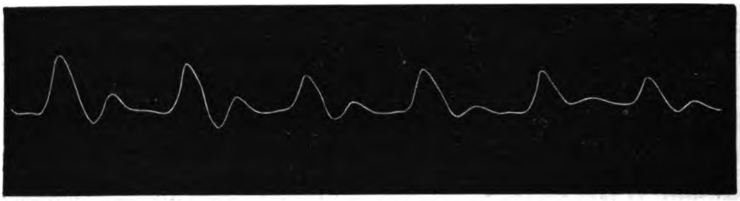
Experimental tracings from schema of elastic tubing, combined with sheep's heart.



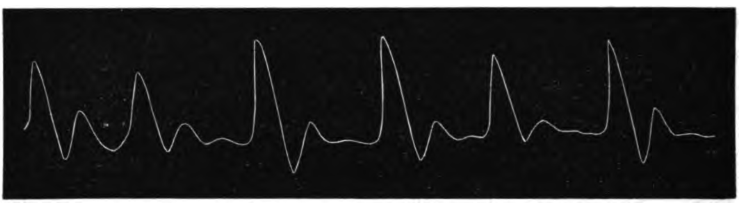
1. Lowest pressure. The dicrotic wave is about as high as the primary, and is followed by other oscillatory waves.



2. Low pressure. The dicrotic wave is still large, and the tracing resembles the hyperdicrotic pulse of fever.

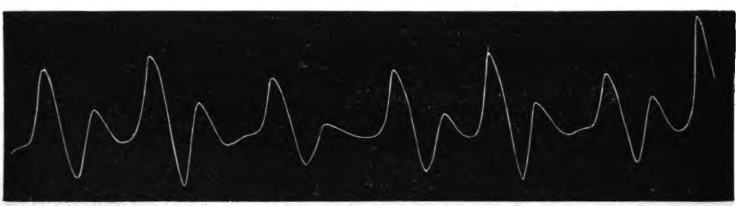


3. Higher pressure. This tracing is less dicrotic, and resembles the pulse of slight fever, or of feeble health.

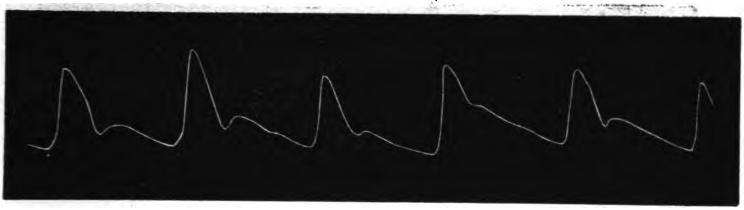


4. One semi-lunar valve divided. The dicrotic wave is less than before the alteration (compare figs. 1 and 2), but still considerable. It is followed by a second oscillatory wave.

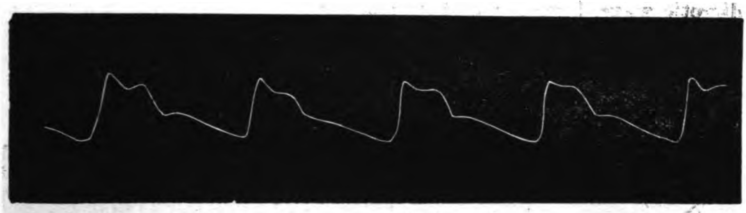
Tracings from the same schema, combined with artificial heart.



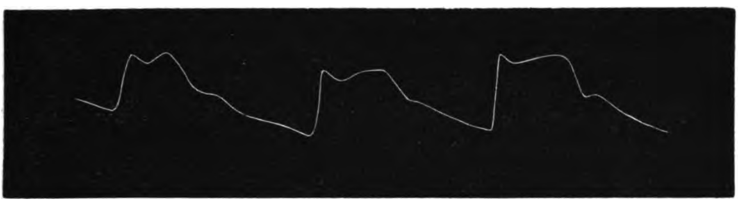
5. Low pressure. This tracing resembles 2, obtained with the sheep's heart, and 13 the pulse of fever, but the rate being slower, a trace is seen of the second oscillatory wave.



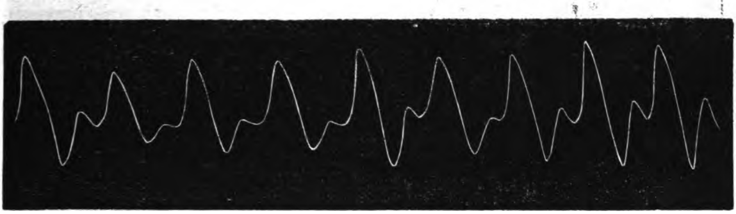
6. High pressure. This tracing resembles one of the forms of healthy pulse.



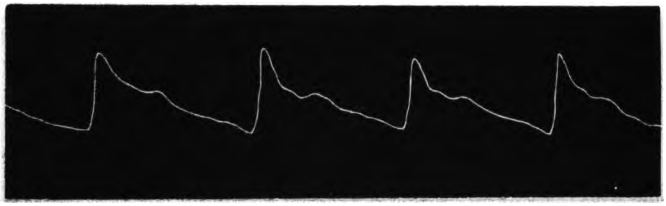
7. High pressure: rigid tubing. The tidal wave here for the first time makes its appearance, and the dicrotic wave becomes less in proportion. This tracing resembles the pulse of atheroma.



8. High pressure: rigid tubing: long contraction. The tidal wave is here much prolonged.

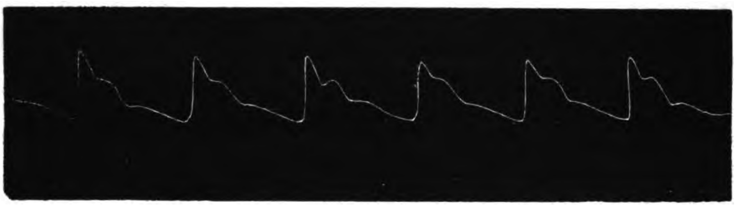


9. Aortic valves entirely removed: pressure low. The dicrotic wave is still considerable, the tidal wave absent.



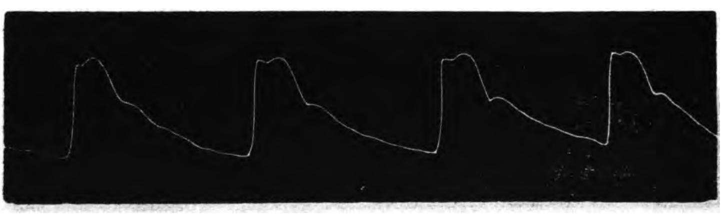
Pressure 2½ ounces.

10. Healthy pulse. The tidal wave is seen preceding the dicrotic wave, but only slightly marked.



Pressure 3½ ounces.

11. Atheroma without kidney disease. The pressure is somewhat greater than normal, the tidal wave large, and the dicrotic wave small in proportion.

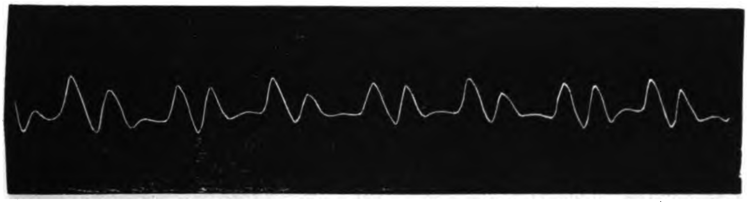


Pressure 5 ounces.

12. Atheroma with granular kidney. The pressure and height of upstroke are both increased, and the tidal wave is very large.

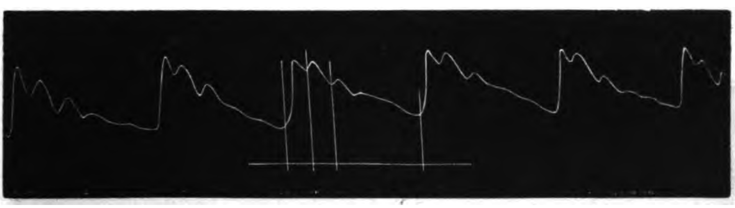
Pressure 1 ounce.

13. Dicrotic pulse of fever. Tidal wave absent; dicrotic wave large: pressure low.



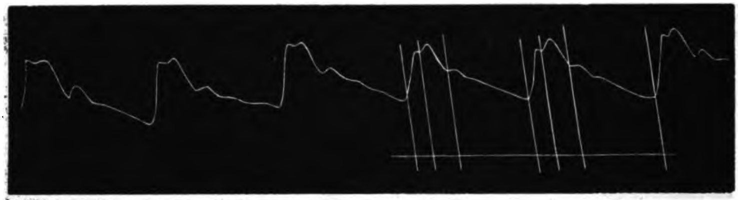
Pressure ½ ounce.

14. The pulse of a case of Bronchitis with chronic Alcoholism taken a few hours before death. The pressure is excessively low: the tidal wave absent: the dicrotic wave almost equal to the primary, and followed by a second oscillatory wave, which is hardly ever seen in the human pulse.



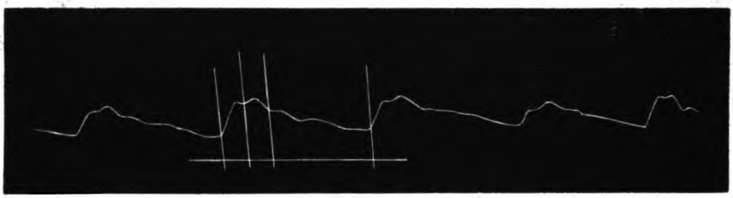
Pressure 5 ounces.

15. G. L. aged 18. Granular kidney. Tracing of radial pulse. The pressure is very high, the tidal wave large and distinctly separated: after it is seen the dicrotic wave, and after the dicrotic wave a third secondary wave, which is the third oscillatory wave of the sphygmograph, the second such wave being superposed on the dicrotic wave.



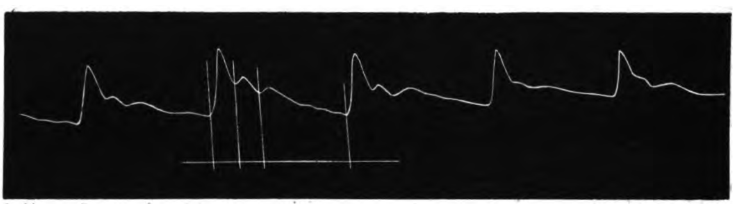
Pressure 5 ounces.

16. G. L. Radial pulse. Lever of sphygmograph weighted. The tidal wave is thus brought nearer to the primary wave, while the dicrotic wave retains about the same relative position.



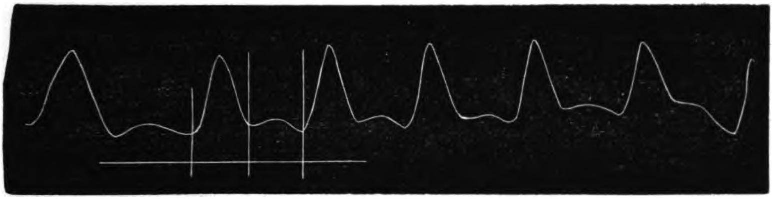
Pressure 4 ounces.

17. G. L. Tracing from brachial artery.



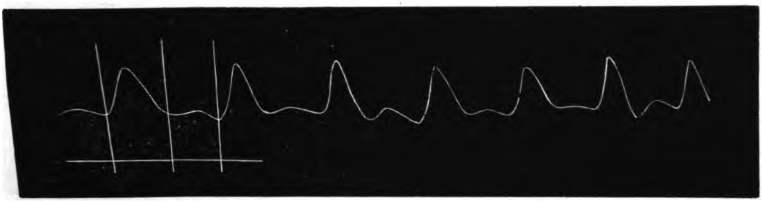
Pressure 4 ounces.

18. G. L. Tracing from dorsalis pedis. The tidal wave is not more widely separated from the primary wave than it is in the brachial or radial arteries, but, if anything, rather the reverse.



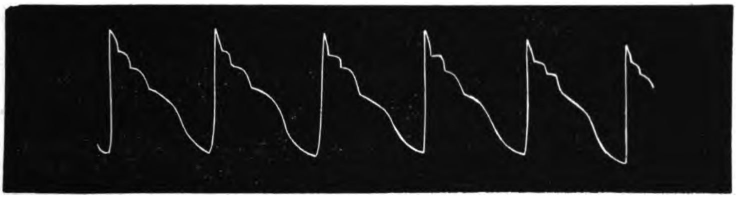
Pressure 3 ounces.

19. Tracing of dicrotic pulse from the femoral artery just below Poupart's ligament.

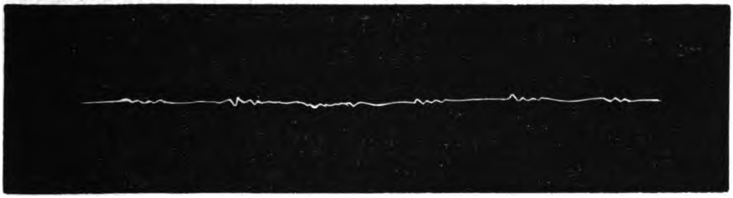


Pressure 3 ounces.

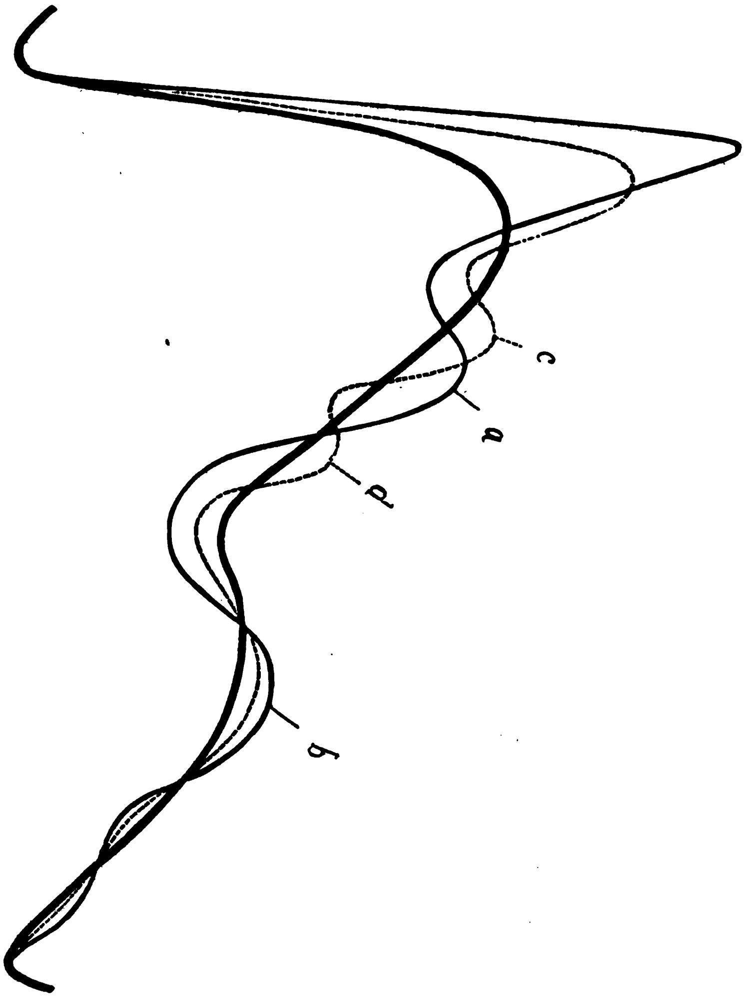
20. Tracing from the posterior tibial artery below the ankle of the same person. The dicrotic wave is not nearer in proportion to the primary wave, as it would be if it were a reflected or retrograde wave.



21. Tracing copied from M. Marey, shewing how the tidal wave is broken into two small waves by the use of a secondary spring in the sphygmograph.



22. Tracing shewing the effect of percussion on the exterior of a rigid part of the experimental tubing.



23. Diagram to illustrate the relation of the sphygmographic tracing to the true pulse-wave. The thick line is intended to represent the true pulse-wave, the thin line the sphygmographic tracing, the dotted line the tracing drawn by a sphygmograph having a secondary spring to keep down the lever.

a. Tidal or "first secondary" wave.

b. Dicrotic or "principal secondary" wave.

c. *d*. Two small waves into which the tidal wave may be broken by the action of the secondary spring, as shewn in fig. 21.

ON THE AMYLOLYTIC FERMENT OF THE PANCREAS.

By ARCHIBALD LIVERSIDGE, F.C.S., *late Scholar of Christ's College, Cambridge.*

(*From the Physiological Laboratory in the University of Cambridge.*)

THE (so-called) ferment of the pancreas was chosen as the object of the following study rather than that of the saliva, simply because it could be obtained much more readily in sufficiently large quantities.

1. *Mode of Preparation.*

The method of V. Wittich (*Pflüger's Archiv*, II. p. 193) was selected as affording a means of obtaining the ferment in as pure a form as possible in quantities sufficiently large.

Fresh pig's pancreas having been freed from fat and finely minced, the pulpy minced mass was placed in a flask and covered with spirit; at first ordinary strong methylated spirit was employed, but afterwards, so as to render the proteids more completely insoluble, absolute alcohol was used. After standing for a few days the spirit was removed by straining through fine muslin—the residue was next treated with pure strong glycerine for 24 or more hours (sometimes the glycerine was left on the pancreas for a month even). The glycerine extract was separated by squeezing the mass through muslin, and then clarified by allowing it to slowly filter through thick flannel bags (after many long and patient trials of other methods, with filters of various kinds and with the use of Bunsen's aspirator, this plan was finally adopted, for it was the only one which gave a clear and transparent glycerine extract). The residue was again similarly treated with three or four additions of fresh glycerine until an appreciable quantity of the ferment was no longer extracted. The filtered clear glycerine extract thus obtained was then treated with many times its bulk of strong spirit in large glass cylinders; after standing for some time the clear supernatant fluid was syphoned off from the white flocculent precipitate. This precipitate, when the alcohol had

been removed by careful filtration and drying at a low temperature, was partly soluble in distilled water. The filtered aqueous solution thus obtained was extremely active in converting starch into sugar. But concentrated solutions (i.e. solutions obtained by treating large quantities of the precipitate with small quantities of distilled water) always gave most unmistakable proteid reactions (xanthoproteic reaction and colouration with Millon's reagent). I never succeeded in getting this first glycerine extract free from proteids.

To further purify the ferment thus prepared, the above precipitate was taken, washed with strong clean spirit, and after partial drying, by the spontaneous evaporation of the spirit, was again treated with glycerine, and this second glycerine extract was in turn precipitated by spirit.

This second precipitate was far less in bulk than the first. Its aqueous solution had about the same activity in converting starch into sugar as that of the first extract, while, though the precipitate itself still gave some evidences of proteids being present, the solution gave hardly any appreciable reactions of them.

2. *The Composition of the Ferment.*

The ferment prepared as above by making two successive glycerine extracts was dried at 100° C. On boiling with KHo a small quantity of ammonia was evolved.

Ash. Heated on platinum foil the ferment intumesced and evolved copiously a gas which burnt with a clear luminous flame, and emitted a slight nitrogenous odour. A bulky coke was left, but burnt off very readily and left a white very fusible ash.

I was prevented by my departure for Australia from completing, as I had intended to do, the analysis of the ferment thus prepared, but Mr Frank Clowes was kind enough to make for me the following determinations of the carbon, nitrogen, and ash. Two determinations of each were made, ·05 gramme being used in each trial.

	I.	II.	Mean.
Carbon	·017611	·017314	·017462
Nitrogen	·005521	·005499	·005510

Ash ·1147 grm. gave ·0174 ash.

Taking the mean we have in percentage,

Carbon 34·925, Nitrogen 11·020, Ash 15·170.

A specimen of the ferment which had been prepared by a single glycerine extraction only, gave in ·05 gramme,

Carbon ·022963, Nitrogen ·006884,

or in percentage,

Carbon 45·926, Nitrogen 13·768.

A quantity of *pepsin* prepared in an exactly similar manner by glycerine extraction from pig's stomach gave, also in ·05 gramme in two trials,

	I.	II.	Mean.
Carbon	·019199	·0205927	·0198958
Nitrogen	·004995	·0051115	·0050532

Ash ·1177 grm. gave ·0194 of ash,

or in percentages,

Carbon 39·792, Nitrogen 10·106, Ash 16·48.

I do not think that any great importance can be attached to these analyses. The material was certainly, in spite of my care, mixed with some amount of foreign substance (filter fragments, &c.); and, independently of this, one would be very rash to infer that the substance obtained, even after a second treatment with glycerine, was ferment and nothing else. The large quantity of ash (principally potassic phosphate apparently) will alone serve to indicate this. Still it is not uninteresting to observe that the pepsin and pancreatic ferment have a very similar constitution, widely different from that of ordinary proteids. My own preliminary determinations had indicated a still lower percentage of nitrogen and of ash.

3. *Action of an aqueous solution of the ferment upon iodide of starch.*

Iodide of starch is almost immediately decolourized on adding an aqueous solution of pancreatin, and the same quantity of ferment will decompose successive quantities of iodide. This decomposition takes place in the presence of free starch;

for on adding more iodine to the decolourized solution it again acquires its blue colour. On adding potassium nitrite (KNO_2) and a drop of sulphuric acid the iodine is set free and the blue restored. Hydroxyl[1] also does this. But in neither case is the restored blue quite so intense as the original. Chlorine water does not answer well for this reaction. After standing for about 30 minutes the restored blue disappeared again, shewing that the body, active in splitting up the iodide of starch, was still unchanged.

There is then in the purified ferment solution a something which splits up iodide of starch and brings the iodine into a combined condition. V. Wittich is inclined to attribute importance to this reaction as throwing light on the mode of action of the ferment. But I always obtained the same reaction with boiled ferment which had completely lost its power of transforming starch into sugar. Blood serum and other proteids, as is well known, also decompose the starch iodide, and I find that the action is quite similar to that of the ferment, being restored by potassium nitrite in presence of an acid, &c.

4. *Action of pancreatic ferment on other substances.*

The modus operandi of an amylolytic ferment being the adding on of a molecule of water, it seemed possible that the ferment might be induced to bring about not only the particular transformation of starch into glucose, but also other transformations which consist in the addition of a molecule of water. The results were entirely negative, but are perhaps worth recording.

Salicine was tried first because the statement that saliva and other amylolytic ferments will convert salicine into saliginin and glucose is often found in text-books.

From a very large number of experiments it was found that an aqueous solution of pancreatic ferment did not decompose salicine into glucose and saliginin. A temperature of 40° C. was used, and the time allowed was from 24 to 48 hours. The pancreatic solution was always previously proved to be good by allowing it to act upon starch paste; while, on the other hand, if a solution of salicine was left for a longer period, even in a

[1] The com[l]. 10% solution.

stoppered bottle, more or less saliginin and glucose were produced from its so-called spontaneous decomposition.

Large quantities of the ferment were tried, but with no result. Hence it seems probable that this spontaneous splitting up of salicine has been referred to the action of amylolytic ferments.

It was thought, however, possible that the action which did not take place in a mixture of the ferment and salicin alone, might be brought about if the ferment were first of all thrown, so to speak, into an active condition by the presence of starch; that the action of the ferment on the starch might be carried on to the salicin. To a quantity of starch in active change under the influence of pancreatic ferment, salicin was added, but no saliginin was found at the end of 48 hours. On the third day a little was found, but by that time the whole mixture had become decomposed, and was covered with penicilium.

The same negative results were met with in attempting to produce change in urea, tartaric, and oxalic acids, both alone and in the presence of starch in active change.

The effect of the ferment on indigo blue was also tried with like negative results.

The purified solution of pancreatic ferment, like all impure amylolytic and other ferments, decomposed hydric peroxide with evolution of oxygen; and the action was wholly absent when the solution of the ferment had been previously well boiled.

5. *Upon the regeneration of the ferment in the previously exhausted pancreas.*

Over some minced pancreas which had been twice extracted with glycerine a stream of water was passed for 12 hours in a tall glass. The washings still contained sufficient ferment to decompose starch paste; this was ascertained by allowing the washed residue to stand in a small portion of the washing water for 30 minutes and then trying its effect upon starch paste. But on continuing to wash for a longer period the whole of the ferment appeared to be removed, for the residue ceased to render a small quantity of water active. This washed and inactive residue was now transferred to a filter and allowed to remain on it exposed to the air for six hours: when on again

treating it with a small quantity of distilled water it was found to render the water very active. One and the same portion of pancreas was in this manner deprived of its ferment four successive times, and each time the ferment reappeared. This form of experiment was repeated on four different portions of pancreas.

As a crucial experiment two quantities of pancreas were placed in April, 1871, in 4oz. flasks and the flasks filled up with glycerine; after standing some time the glycerine was filtered off (i.e. squeezed through muslin) and fresh added; even after the seventh application of glycerine the pancreas still yielded ferment. Altogether, these two quantities of pancreas were treated with eleven successive doses of glycerine, and were not rendered free from ferment until June, 1872, i.e. after standing in comparatively large quantities of pure strong glycerine for fourteen months.

Glycerine extract from flask No. 1, after standing on the pancreas for eight days from the time of last filtering and washing (for it should be recorded that not only was the glycerine removed in each case by squeezing through muslin, but also the residue was well washed under the tap for some time, so as effectually to remove every trace of glycerine), was, on June 3, 1872, at last found to be inactive. Yet this same residue, after standing on the muslin filter for six hours, readily gave not only an active aqueous extract, but also yielded an active glycerine solution.

On June 3rd the glycerine extract from flask No. 2 was slightly active, but the residue after one more washing became inactive, i.e. did not yield any ferment to glycerine, and yet after six hours' exposure to the air it readily did so. As the washed pancreas decomposes it evolves an odour of rotten cheese, and the aqueous extract is then very active upon starch paste. These experiments seem to shew pretty conclusively that the ferment is regenerated in the exhausted pancreas.

The assertion of Bernard and others, that all decomposing proteids are amylolytic has been disproved by Dr M. Foster (see *this Journal*, 1, I. 107); and we may fairly infer that there is in the pancreas some substance (or substances), not in itself active as a ferment, which in the processes of decomposition

becomes converted into the ferment. It is worthy of note, that though the solid washed tissue of the pancreas thus readily gave rise to ferment, the aqueous solution of the ferment itself as obtained by the glycerine method, when once its amylolytic powers had been destroyed by boiling, never regained them afterwards, at any stage of the decomposition which subsequently took place in it.

It was my intention to have attempted the isolation of this antecedent (or antecedents) of the ferment, but my departure for Australia put an end to my studies in this direction.

SYDNEY, *Nov.* 1872.

ON THE STRUCTURE OF THE TACTILE CORPUSCLES. BY GEORGE THIN, M.D. (Plates I., II.)

THE structure of the tactile corpuscles of Wagner is a subject on which anatomists are not agreed. It is unanimously admitted that one or more medullated nerve-fibres have a course through the corium to the corpuscle with which they come in contact, that the corpuscle itself presents a well-defined contour, that on its surface are to be observed a number of rounded or oval bodies—the so-called transverse elements (*quer-elemente*) of German authors—which by their more or less parallel arrangement and number contrast with the cellular elements in the contiguous tissue, and finally that, when subjected to the influence of the agents commonly employed to prepare the skin for microscopic examination, the substance of the corpuscle seems to be composed of waving fasciculi of fibrillæ. The points on which observers are not agreed are the fate of the nerve after it comes in contact with the corpuscle, the nature of the transverse elements, and of the substance of which the corpuscle is composed.

Dr Allen Dalzell presented to the University of Edinburgh in 1853 an "Inaugural Dissertation on the General Integuments of animals and their appendages[1]," in which he records the results of his examination into the nature of the touch-corpuscles as follows:—"In very thin sections of integument, and when the knife had evidently been carried in the same perpendicular as the nerves, a power of 400 linear with acromatic light from a Ross condenser never shewed the division (either within or without the corpuscle) of the double contour nerve into pale filaments; an appearance often observed by Wagner, twice confirmed by Gerlach, but never detected by either Nuhn or Kölliker." And again, "In touch-bodies with a hyaline centre no nervous structures had been seen in section, but repeatedly in those in which the so-called 'cortical appearance' was continuous throughout the touch-body, the appearance of two or more circular apertures had been detected which might be the cut ends of tubes."

[1] This Thesis is a manuscript volume in the Library of the University. It has never been printed.

Tomsa (*Wien. Med. Wochenschrift*, 1865) experimented by boiling the skin in a mixture of alcohol and hydrochloric acid. The theory of this procedure is that it leaves intact the axis-cylinder, whilst the surrounding elements are dissolved. He states as the result of his researches, that the nerve-fibre gradually loses its medulla as it approaches the corpuscle, and making a number of spiral windings before entering it, "splits up into a varying number of branches whose section presents a polygonal contour." The corpuscle itself is, he holds, formed by cellular elements more or less transversely disposed. "These flattened cells" are through their very short prolongations continuous with each other and with the fibrillæ of the axis-cylinders which enter the pedicle of the corpuscle." To other corpuscles he assigns a different composition. "Other tactile corpuscles in the palmar surface of the hand are composed of a coil of thickened axis-cylinder, an aggregation of nerve-material, in which are embedded nuclei which are either transversely arranged at the periphery, or are irregularly distributed over the surface. The prolongations of the cells are here in the background, and the corpuscle consists entirely of a compact mass of nerve-substance formed by the confluence of the cell-protoplasma of the nerve." He states emphatically that the cellular elements of the corpuscle do not belong to connective tissue elements.

Kölliker[1] describes the corpuscle as composed of a cortical layer, a nucleated capsule, and an interior of homogeneous clear connecting substance with fine granular elements. He cites the opinion of Krause that the transverse elements are nerves, and maintains, in opposition to him, that they are transversely disposed nuclei (*querstehende Kerne*). In regard to their nature, he states that "they perhaps all belong to cells similarly arranged, to which the value of connective tissue corpuscles might be assigned." But he also adds—"that there are often also in addition transversely disposed nerve-fibres in considerable number is certain, but these are not the chief cause of the transverse linear appearances." In regard to the nerve he believes that one to two, or even three to four, nerve-fibres run along with or surround the corpuscle, and then "apparently" enter it, "ending in the superficial parts of the inner substance in pale terminal fibres." He further states, "but it would seem that the nerves end in the superficial parts of the corpuscle and never pass through its centre."

Biesiadecki (in Stricker's *Handbook*) refers briefly to these points as follows: "The transverse lines have been differently explained as connective tissue, elastic, and nerve-fibres, the transverse nuclei sometimes as connective tissue cells, sometimes as the nuclei of the membrane of Schwann. According to some, after the disappearance of the medulla the sheath of the nerve enters a depression in the corpuscle, in which it has a free ending, analogous to the corpuscles of Krause in other situations.

"Successful gold preparations decide some of these doubtful

[1] *Handbuch*, 5th Edition.

points, as they shew the nerve-fibres coloured dark violet, whilst the remaining tissue appears of a pale reddish hue. The margin of the corpuscle is indicated by a faintly marked contour in which oblong nuclei lie. In fine sections 4—6 violet-coloured nerve-fibres can be seen which are sometimes obliquely, sometimes longitudinally arranged, and which are accompanied by more faintly coloured small nuclei.

"But these fine sections teach nothing concerning the course of the nerve in the interior of the corpuscle; they give no explanation as to whether the fibres divide and as to how they end."

Virchow (*Cellular pathologie*, 1871, p. 284), after enumerating the various opinions held regarding the relations of the nerve to the corpuscle, and expressing his conviction that Meissner is in error in considering the corpuscle to be composed of nervous matter, remarks, "It seems to me to be a matter of doubt whether the nerve ends in the interior of the corpuscle or forms a loop in its circumference."

My investigations, most of which were conducted in the spring of this year, in Professor Stricker's Laboratory for Experimental Pathology in Vienna, have yielded me the results which I have now to describe [1].

The methods I employed consisted chiefly in treating freshly amputated skin by Chloride of Gold, Osmic Acid, and Carmine and Acetic Acid.

Osmic acid has the great advantage of not shrinking the tissues, and, when employed successfully, of giving remarkably distinct and perfect specimens. *Tactile corpuscles consist of two classes, Single and Compound.* The greater number, inclusive of all the larger ones, belong to the latter category.

A vertical section through the meridian of a corpuscle which has been properly treated by Osmic acid, shews either a simple homogeneous, more or less, rounded body, enclosed in a capsule, or two or more such simple capsulated bodies arranged in a row, parallel to the vertical axis of the papilla, and enclosed in a common oblong capsule. The former I propose to designate as Single, the latter as Compound Corpuscles. Compound Corpuscles may again be conveniently divided into twins and triplets, according as they are composed of two or three single corpuscles, and for convenience in description, each of these individuals may be termed a member of the Corpuscle. Exam-

[1] Many of these results I submitted to the Academy of Sciences in Vienna.—(*Sitz.-Bericht.* May, 1873.)

ples of Singles will be found in Plate I., Figures 3, 6, and 10, of Twins in Figures 7 and 8, of Triplets in Figures 9 and 11. The space which separates the members of a compound corpuscle varies in breadth (Figures 7, 8, 9 and 11). The compound corpuscle is thus a conglomeration of Singles, and must be regarded not as one, but as consisting of several organs.

The well-marked unmistakable separation between the members of compound corpuscles I have been able to demonstrate only in osmic acid preparations, and then only when the section happened to be through the meridian of the corpuscle. If the section has laid bare the capsule of a compound corpuscle, the line of demarcation between its members can only, even in osmic acid preparations, be inferred from the depressions in the capsule (Figure 3). In gold preparations the depressions in the contour corresponding to the boundaries of the members are sometimes visible as in Figure 1, and sometimes not as in Figure 2, probably according as more or less shrinking has been produced in the preparation of the tissue for examination. In whatever way the skin is prepared the bulging of the members of compound corpuscles is often seen (Fig. 12).

On account of the winding course of the nerve it is in the majority of sections only seen in detached portions. Occasionally no nerve is seen, which happens when the section has hit the external portion of the corpuscle. When the preparation is successful, the nerve-fibre is seen with perfect distinctness within the substance of the corpuscle, the extent to which it is visible depending on how far the direction of the section has been parallel to the course of the nerve.

The result of the examination of a very large number of specimens has shewn me that each single corpuscle, and each member of a compound corpuscle, is penetrated by one, and never by more than one, nerve-fibre.

I have never seen a nerve leave the substance of the corpuscle after having penetrated it.

Although a perpendicular course in the corium is the rule for the nerve-fibres that terminate in the tactile corpuscles it is not invariable. In triplets especially it is not rare to find

that the nerve of the upper member approaches it by winding transversely from the summit of another papilla.

That the nerve penetrates the substance of the corpuscle is demonstrable both in gold and in osmic acid preparations. Before piercing the capsule it sometimes describes a curve, or a complete spiral, or it may suddenly twist round itself and form a loop, or it may simply enter in a straight line.

If the corpuscle is single, the nerve, as soon as it has traversed the capsule, at once penetrates into its interior (Fig. 10). If it is compound the nerve after it has penetrated the capsule either immediately enters the member opposite its point of entry, or takes an upward course within the common capsule towards the member to which it is destined, mostly parallel to the vertical axis of the papilla, but it may in its course cross the corpuscle once or even twice transversely, and generally in the grooves between the component members (Fig. 11). In its course towards the member in which it terminates it follows no fixed rule, sometimes running parallel to the long axis of the corpuscle inside, or outside the common capsule, and sometimes, as has been indicated above, approaching it in a line parallel to the transverse axis of the papilla and on a level with its point of entry to the capsule substance.

In gold preparations it can be distinctly seen that in compound corpuscles a varying number of nerves enter at different altitudes, and in osmic acid preparations it is seen that the points of entry correspond to the relative position of the members of the corpuscle.

When the nerve has entered the substance of the corpuscle it penetrates to a certain depth either in a straight line or in a curve, and then bends round and describes part of a circle. The largest curve I have seen has not exceeded an arc of 270°. In this terminal course the nerve retains its medulla, and between the medulla and the corpuscle substance there is a clear space visible in the osmic acid preparations (well seen in Fig. 10) which exactly corresponds to the position of the sheath of Schwann.

I have not once seen the nerve-fibre divide either external or internal to the capsule, and the number of preparations in which I have obtained a clear view of the nerve before and

after its entry through the capsule is so considerable that I do not hesitate to deny that the alleged division of the medullated nerve in the tactile corpuscle exists[1].

The inference from these facts is that each single corpuscle and each member of a compound corpuscle represents the termination of a single medullated nerve-fibre.

The transverse elements, seen with varying distinctness according to the manner in which the skin is prepared for examination, are especially prominent in gold and in acetic acid preparations. They are best demonstrated by making a thin section from fresh skin, colouring it in carmine, and then allowing it to macerate in concentrated acetic acid for about 48 hours. Fig. 12 is drawn from a section so prepared.

By teasing gold and carmine and acetic acid preparations, I succeeded in isolating fragments of corpuscles, and was able to demonstrate the accuracy of the conviction I had acquired from examining macerated specimens, that the transverse elements are the nuclei of oblong cells which anastomose with each other by means of prolongations of elastic tissue fibres. The distinction between the cell and the nucleus is not often accurately defined, but I have seen it repeatedly and unmistakably both in gold and osmic acid preparations. The cells colour dark in gold, which, in conjunction with their elongated form, gives them an appearance which it is sometimes difficult to distinguish from that of nerve-fibres. They are very abundant in every part of the corpuscle, which may be described as being in great part formed by a dense network of cells and elastic tissue. The cells and elastic fibres seem to be as numerous in the deeper as in the superficial parts, every change of focus in a suitably prepared section bringing a new layer into view.

[1] Appearances are often seen both in the corium and within the capsule that might easily be mistaken for the division of a nerve-fibre. For example, in Figure 2 three nerve-fibres are seen ascending towards a corpuscle, and in one part of their course one of the fibres lies so directly under another that only two fibres are then visible. If in such a case the section did not happen to include the deeper portion of the course of the fibres the illusory image might be presented of two nerves in the upper part of the corium apparently becoming three before entering the corpuscle.

In Figure 5 a second nerve is seen emerging at the side of a corpuscle almost immediately under a first that is seen ascending through the corium, and so producing an appearance that might be mistaken for a division of the first nerve.

In a successful attempt to colour the skin with silver, I found the position of the corpuscles indicated by oval masses of beautifully distinct granulated nuclei, which changed by the slightest touch of the fine adjustment.

The capsule of the corpuscle is formed by a circular layer of elastic tissue formed by the anastomosing continuations of cells.

The network of elastic tissue in the corpuscle and the cells connected with it communicate in no way with the medullated nerve-fibres.

The connecting matter of the corpuscle clears up with acetic acid, and is coloured by gold and osmic acid the same shade as the connective tissue of the surrounding corium.

The division of the papillæ of the skin into vascular and nervous, I have not found borne out by my investigations.

Virchow (*Cellular-pathologie*, Berlin, 1871, p. 283) remarks[1], "In the narrower papillae there is always a simple or dividing vascular loop but no nerve. This observation is in so far important as through it we have acquired the knowledge of a new nerve-less part. In the other kind of papillæ, on the contrary, very frequently no vessels but nerves and the peculiar structures designated as touch-corpuscles are found."

Again (*Op. cit.* p. 285) he remarks, "Setting aside the anatomical and physiological question, the instance of the skin papillae has a great value in explaining pathological appearances, because we find here in parts in themselves perfectly alike two conditions that are in absolute contrast[2]: *one set of papillæ that have no nerves and are rich in vessels, and another set that have no vessels and are only provided with nerves.*"

Isidor Neumann, in his text-book of Skin-Diseases (Second Edition), in the Chapter on the Anatomy of the Skin, represents the general opinion of German anatomists when he remarks that "Compound papillae only, and these only rarely, contain both a touch-corpuscle and a vascular loop; with this exception papillæ with corpuscles have no vessels."

Dr Dalzell had already in 1853, in the Thesis from which I have quoted, expressed himself in the following clear and decided terms—"Wagner's division of the papillæ into purely nervous

[1] In den schmalen findet man constant eine einfache, zuweilen eine verästelte Gefäss-schlinge aber keinen Nerven. Es ist diese Beobachtung insofern wichtig, als wir durch sie zur Kenntniss eines neuen nervenlosen Theiles gekommen sind. In der anderen Art von Papillen findet man dagegen sehr häufig gar keine Gefässe, sondern Nerven und jene eigenthümlichen Bildungen, welche man als Tastkörper bezeichnet hat.

[2] Einerseits nervenlose und gefässreiche, andererseits gefässlose nur mit Nerven versehene Papillen.

and vascular has no anatomical foundation either in man or the lower animals, and even those papillae in which his corpuscula tacti are placed very frequently contain one or more vessels."

I have satisfied myself that when skin from a finger that has been amputated whilst the capillary vessels are full of blood is treated with osmic acid, nerves, touch-corpuscles, and rows of blood corpuscles arranged in loops indicating the position of the capillary vessels, are distinctly seen in the same papillæ. (Plate II., Figs. 13 and 14.) It is capable of easy demonstration that in a majority of so-called nerve-papillæ a thin vertical section contains at the same time a section of the corpuscle and one or more capillary loops. In the smaller number in which nerve or touch-corpuscle is seen and no capillary loop, it is quite possible that in the wide papilla the vessel may not have been included in the section. What is certain is, that at least the majority of papillæ which contain touch-corpuscles contain vessels also, and that the distinction of papillae into nervous and vascular has no foundation in fact. Nerve-fibres are found in papillæ which have vessels and no touch-corpuscles, and in a proportion that seems to bear a relation to the number of fibres found approaching the rete from the corium in a given area.

EXPLANATION OF PLATES.

Plate I. Figures 1 and 2 are compound corpuscles prepared by treatment with Chloride of Gold. The nerves are only visible to their point of entry.

Figure 3 is part of a compound papilla prepared by Osmic Acid, showing two corpuscles with part of the course of the nerve in their interior, and with part of a capillary loop indicated by blood corpuscles.

Figures 4 and 5 are compound corpuscles prepared by Chloride of Gold, showing the course of the nerves in the interior of the corpuscles.

Figure 6 is a single corpuscle prepared by Osmic Acid.

Figure 7 is a twin corpuscle prepared by Osmic Acid. Two nerve-fibres are seen on one side of the corpuscle and a capillary

vessel filled with blood corpuscles on the other. The nerve of the upper member is seen entering the corpuscle substance.

Figure 8 is a twin corpuscle prepared by Osmic Acid.

Figure 9 is a triplet corpuscle prepared by Osmic Acid, with sections of two nerve-fibres.

Figure 10 is a single corpuscle prepared by Osmic Acid, with its nerve lying in its substance.

Figure 11 is a compound corpuscle prepared by Osmic Acid. The nerve to the upper member is seen crossing in the grooves between the members under the common capsule before entering.

Figure 12 is a compound corpuscle which has been coloured with Carmine and macerated in Acetic Acid, showing the nuclei of the cells and the continuity of the latter with elastic fibres. (The relative position of the figure in the plate is inverted.)

Plate II. Figures 13 and 14, from Osmic Acid preparations, illustrate the co-existence of touch-corpuscles and vessels in the same papillae. The figures represent vertical sections through the skin. c. c. capillary blood-vessels; n. nerve-fibre, t. touch-corpuscle.

NOTES ON THE STRUCTURE OF THE OLFACTORY MUCOUS MEMBRANE. By H. NEWELL MARTIN, D. Sc., M.B. Lond., *Scholar of Christ's College, Cambridge.* (Pl. III.)

(From the Physiological Laboratory in the University of Cambridge.)

I. THE EPITHELIAL CELLS.

SINCE Max Schultze first described two forms of epithelial cell as occurring in the olfactory region of the nasal mucous membrane, and divided them into the two classes of *epithelial* or supporting cells, and *olfactory* or special sensory cells, most subsequent observers have confirmed him on every essential point. Recently, however, Exner[1] has denied the distinctness of the two forms of cell described by Max Schultze, and maintained that they gradually shade off into one another, numerous transitional forms being found between them. The following notes are based on observations which I have not been able as yet to carry on to a systematic conclusion, but which, so far as they go, lead me to confirm decidedly the anatomical distinctness of the two forms of cells described by Max Schultze, whilst, at the same time, they verify in very many points the accuracy of Exner's descriptions. The animals with which I have principally worked are the newt and the dog, but I have also employed the frog, rabbit, guinea-pig, and rat.

Olfactory Epithelium of the Newt.

In this it is quite easy to make out two distinct kinds of cell, and I have never succeeded in discovering any intermediate forms. In the newt, indeed, the two forms of cell are more widely different than in any other of the animals mentioned above; it affords therefore an excellent subject on which to begin the microscopical study of this tissue.

If the olfactory mucous membrane of this animal be treated with Müller's fluid, and then teased carefully out in water,

[1] *Sitz. der K. Acad. der Wissensch. Wien. I. Abth. Jänner-Heft*, 1870, *u. III. Abth. Jänner-Heft*, 1872.

besides isolated cells floating about the field, there will be found, here and there, groups of cells, each group consisting of one of the so-called "epithelial" cells, surrounded by a number of "olfactory" cells (Fig. 1). This appearance is so frequent that I believe the whole epithelium in its natural state to be made up of groups of this kind; so that its surface might be divided into a great number of small areas, each of which would consist of a central "epithelial" cell, with a number of "olfactory" cells grouped round it. This arrangement is still more easily demonstrable in preparations made by hardening the membrane in alcohol, and then teasing it out in glycerine. The groups of cells thus obtained form much more coherent masses, the "olfactory" cells appearing imbedded in a uniting granular substance, which is apparently dissolved away by Müller's fluid (Fig. 2).

When the *epithelial cells* (Fig. 3) are obtained from Müller's fluid preparations, each possesses a large oval granular nucleus, which sometimes contains a few large particles looking like oil globules. Around this nucleus lies a homogeneous structureless layer, with well-defined inner and outer margins; from one end of this layer proceed, sometimes one, but usually several "central processes," which are also homogeneous and structureless. These processes often branch several times, presenting here and there little angular enlargements which, but for their angularity, might be called varicosities, and appear like prolongations of the layer round the nucleus, no line of demarcation being observable between them and it. The "peripheral process," arising from the opposite end of the cells, is, on the contrary, always single, and distinctly marked off from the layer surrounding the nucleus. It is much stouter than the central process, finely granular, and often obscurely longitudinally striated.

By staining these cells with iodine after treatment with Müller's fluid, I have frequently been able to observe in the peripheral process a central stained part, surrounded by a delicate unstained loosely fitting outer membrane, as if the middle part had shrunk away from a cell-wall (Fig. 4). I have never succeeded in finding cilia on the peripheral process of the epithelial cells, as described by Exner; but I have several times met with the appearance shewn in Fig. 5, the central mass

being produced at one side of its free end into a tolerably thick prolongation.

The nucleus of these cells varies considerably in appearance with the reagent employed in preparing the membrane. In gold-chloride preparations it is granular, as in Müller's fluid; but in chromic acid it is hardly granular at all; and, judging from analogy with the dog, it probably appears non-granular in osmic acid preparations also.

In some few cases (Fig. 4) it has seemed as if the central processes were replaced by a delicate crumpled membrane; but I am inclined to believe that this is an optical illusion. Babuchin[1] has, however, described these cells as having such a membrane at their deep end, and says that he has stained it.

The *olfactory cells* in Müller's fluid preparations exhibit a spherical nucleus very distinct from the oval one possessed by the "epithelial" cells. This nucleus has a peculiar appearance —it is not *granular* in the ordinary sense of the term—but looks as if some highly refracting body were wrinkled or broken into fragments so as to refract the light unequally and get a sort of pseudo-granular appearance (Fig. 1). It is surrounded by a hyaline transparent layer, exactly like that round the nucleus of the "epithelial" cells; and from this proceed in many cases a peripheral and a central process, both of which are hyaline and transparent. The peripheral one is rather the thicker of the two. The central one exhibits varicose enlargements, but does not divide, like the central processes of the epithelial cells.

Many cells are always to be found about the field resembling in most points those just described, but differing in having no process, or only one. They have usually been considered as mutilated olfactory cells; but the most careful examination so frequently fails in detecting any indication of a broken-short process, that I am inclined to believe that some of them may be normal, and lie naturally imbedded in the network formed by the deep processes of the "epithelial" cells. In some of them the nucleus is double (Fig. 6). Cells resembling the olfactory cells, but having their processes unusual in position or number, are also occasionally found (Fig. 7).

[1] Stricker, *Handb. d. Lehre v. d. Geweben*, Cap. xxxv. p. 969.

Olfactory Epithelium of the Frog.

In this animal the "epithelial" and "olfactory" cells are not so readily distinguished as in the newt. One chief cause of this is, that the nuclei of both are oval, instead of that of "olfactory" cells being round as in the newt. The peripheral processes of the "epithelial" cells also are not relatively so thick, compared with the corresponding processes of the olfactory cells, as in the newt; and the central processes are fewer (usually only one), and less branched. In these three points, then, the two forms of cell approximate to one another; but still, so far as I have seen, they can always be distinguished by the smaller amount of protoplasm around the nucleus of the olfactory cells, by the smaller diameter and less granular character of their peripheral, and by the greater fineness of their central processes.

In some cases I have seen the deeper end of the central process of an "epithelial" cell ending in an enlargement, from which again several small processes arise. This may possibly be an indication of a less developed stage of that peculiar arrangement of the deep ends of the corresponding processes which is seen in the dog. (See Exner's first paper, Taf. I. Fig. 12.)

Olfactory Epithelium of the Dog.

In this animal also the two forms of cells have always appeared to me to be quite distinct; although both differ somewhat from the corresponding cells of the newt.

In the "epithelial" cells the nucleus is smaller and rounder than in the newt; and in osmic acid preparations it is often very indistinct. The central process is comparatively thick, and has little angular prominences here and there upon it. It is always single, and never branches until towards its deeper end. There it swells out into a large knot, exactly as described by Exner in the rabbit; and from this a number of short thick processes arise, whose general direction is in the plane of the mucous membrane, and which appear to join those of neighbouring cells, thus forming close to the sub-epithelial tissue a sort of irregular network with small meshes and thick inter-

secting trabeculæ. Just above its terminal enlargement each process contains a number of granules, which stain very deeply with osmic acid, forming a black patch; and the black patches of neighbouring cells being apposed, form, in vertical sections of the mucous membrane, a black line reaching all along it near its base. This is well shewn in Exner's second paper, Taf. II. Fig. 4. Similar stained granules are usually found in small numbers in the terminal enlargement, and here and there in the central process.

The peripheral process of these cells is essentially like the corresponding process in the newt.

The "olfactory cells" are proportionately less numerous than in the newt; and their central processes are finer. Their nuclei are rather smaller than those of the epithelial cells, and are oval, but rather pointed, at their poles. I have never succeeded in tracing out the deep process of one of these cells to its end, and cannot offer any opinion, as to whether it ends, as Exner has described it in the human infant and some other cases, in the network formed by the deep processes of the epithelial cells. The peripheral processes of these cells are very fine, but otherwise not peculiar.

The conclusion to which I have been led is, that the two forms of cell met with in the olfactory region are anatomically quite distinct, as described by Max Schultze, and do not shade off into one another. I think the contrary opinion at which Exner arrived is due in great part to his having at first and chiefly worked with the frog, where the olfactory and epithelial cells certainly do approximate to one another in several points; although, even in the case of that animal, his own figures do not seem to me to support the view of the existence of such a transitional series of cells as he describes. With all his other statements, so far as I have followed them, I entirely agree.

In my descriptions I have adhered to the names *epithelial* and *olfactory* as those by which these cells are best known, but I am very doubtful whether they possess any such difference of function as is thus implied. Under the best circumstances anatomical structure alone affords a very uncertain basis from which to deduce physiological function; and both these forms

of cell differ so much from those of any ordinary form of epithelium, that there appears to me no reason for ascribing special sensory functions to one more than to the other, and I am inclined to regard them both as concerned directly in the sense of smell. If both end, as Exner has described, in the same deep network, with which fibres of the olfactory nerve are continuous, it would seem to settle decisively the similarity or intimate relationship of their functions.

DESCRIPTION OF THE FIGURES.

Fig. 1. Group of cells from olfactory mucous membrane of newt. Müller's fluid.

Fig. 2. Cells from same hardened in alcohol.

Fig. 3. "Epithelial" cell from olfactory region of newt. Müller's fluid.

Fig. 4. Ditto Ditto. Treated with iodine solution.

Fig. 5. Ditto. Ditto.

Fig. 6. "Olfactory" cells with only one apparent process from newt. Müller's fluid.

Fig. 7. Unusual forms of cell from newt. Müller's fluid.

Fig. 8. "Epithelial cells" from olfactory region of dog. Osmic acid preparation.

Fig. 9. Ditto Ditto Müller's fluid.

ON THE EFFECTS OF A GRADUAL RISE OF TEMPERATURE ON REFLEX ACTIONS IN THE FROG.

BY M. FOSTER, M.A., M.D., F.R.S., *Prælector in Physiology in Trinity College, Cambridge.*

(From the Physiological Laboratory in the University of Cambridge.)

GOLTZ[1] observed that if a brainless frog be placed in a vessel of water, and the temperature of the water be very gradually raised to 40° C., no movements (beyond a few flickering spasms) take place; the frog becomes at last perfectly rigid and dies without any attempt at escape.

An uninjured frog, on the other hand, becomes violent in its attempts to get away as soon as the temperature rises to 30°, or thereabouts.

These observations I have verified repeatedly. They are justly urged by Goltz as a very striking instance of the difference between the conditions of a frog with and without a brain; but they present a new difficulty:—why the brainless frog is not excited to reflex action by the stimulus of the hot water. This difficulty is increased by the following facts.

Obs. 1. If a frog, from which the brain has been removed, be suspended by the jaw with the legs hanging freely down, and the toes dipping into a vessel of water, on gradually heating the water the toes are withdrawn by reflex action as soon as the temperature of the water reaches a little over 30°. The result does not essentially depend on the rapidity of the rise. However slowly the water be heated, the feet are always withdrawn at a temperature of 35°, or earlier. Rapid heating may possibly lower the degree at which the feet are withdrawn; but to this I have not paid particular attention. Whether heated slowly or rapidly the feet are withdrawn at about 35° C. or at a lower temperature.—*Obs.* 2. If the whole body thus suspended be similarly immersed and heated, no movements (or only the very slightest spasms of the muscles of the legs) take place; and on

[1] *Functionen der Nerven-Centren des Frosches.*

still further raising the temperature the body becomes rigid (*rigor caloris*).—*Obs.* 3. If both legs be immersed up to the anus, and similarly treated, they also become rigid without movements either of the legs or of any part of the body, save only a few spasms.—*Obs.* 4. If one leg only be immersed and similarly treated, it also becomes rigid without movements, or with only slight movements.—*Obs.* 5. If both legs (or one leg) be immersed up to the knee, they are sometimes withdrawn; but sometimes no movements take place, and the portion immersed becomes rigid. The results in this case are not so constant as when either more or less of the body is immersed.—*Obs.* 6. If the feet only be immersed, they are invariably withdrawn at 35° C., or under.—*Obs.* 7. If a frog be suspended over a vessel divided by partition, with water at unequal levels on the two sides, so that one leg is wholly immersed and the foot only of the other leg, and the vessel be surrounded with water, the temperature of which is gradually raised, neither the leg nor the foot will be withdrawn, if care be taken that the water on both sides of the partition be equally and uniformly raised in temperature. If in this last observation the water on both sides be reduced to the same level, both feet are withdrawn. This result shews that warm air and vapour have not the same effect as warm water, and that the absence of movements is not due to the unavoidable contact of the thighs of the animal with the top of the partition giving some support to the legs, and thus diminishing the tendency to the withdrawal of the feet.

The above observations shew that when the toes (alone immersed in water) begin to be affected by the high temperature, say 30° C., the stimulus of the hot water causes a reflex action which results in the withdrawal of the foot. When the whole leg or body is immersed, the same stimulus is still at work, but no reflex action occurs. What is the reason that reflex action is absent?

The following explanation is perhaps the first to offer itself. The warmth applied to the leg diminishes the irritability of the nerves or of the muscles, or of both; and thus the impulses generated by the warm water in the sensory terminations of the nerves of the foot are not carried up to the cord owing to the diminished irritability of the sciatic trunk, or, being so carried,

the reflex process taking place in the cord cannot manifest itself on account of the diminished irritability of the muscles or motor nerves.

But this view is clearly untenable. It requires that the nerves and muscles, covered and protected by the skin, should be affected before the sensory terminations in the skin itself. Moreover, no appreciable difference in the irritability of the nerves, trunks or muscles of a leg thus exposed to 35° C., could be detected. And it is directly contradicted by *Obs.* 7, where the immersion of one leg prevents movements in the other.

Two other views then suggest themselves.—(1) The blood returning from the legs being warmer than the normal, raises the temperature of the spinal cord above the normal; this reduces the irritability of the cord, and hence reflex actions set going by a feeble stimulus, which in a normal cord would manifest themselves, are here absent.—(2) From the stimulation of the whole leg as compared with that of the foot, a multitude of impulses, arising from all parts of the skin exposed to the warm water, reach the spinal cord. These produce such an effect upon the cord that the simpler reflex action resulting from the stimulation of the toes alone is prevented.

At first sight it seems easy to separate these two different agencies. For if the reflex excitability of the cord be lowered by the heated blood, it will be lowered all over, and reflex actions will be lessened not only in the immersed legs but in the trunk and fore-legs above the water. Practically, however, in the case of the frog it is extremely difficult to estimate excitability quantitatively by means either of mechanical or of chemical stimuli, in any other part of the frog's body than the legs, when only slight variations have to be accurately determined. With the legs one can estimate by the dilute sulphuric acid method (chemical stimulus) with tolerable accuracy slight variations in excitability; though even here mechanical or electric stimuli are unsatisfactory. But one cannot easily apply the acid method to other parts than the feet, and hence the difficulty. For it is with slight variations that we have here to deal; the legs, which remain perfectly quiet in warm water, are at once drawn up when even slightly pinched or touched.

I attempted to eliminate the effects of rise of temperature

by using some other form of gradually increasing and uniformly applied stimulus.

I tried to do this by using dilute sulphuric acid, the strength of which was steadily increased. The legs were suspended in a small beaker of water, into which water from a very large beaker was continually flowing at a very slow rate, the surplus being removed by a syphon at the same rate. Into the larger beaker dilute sulphuric acid was dropped, with continual stirring. The frog's legs were thus brought in contact in as uniform a manner as possible with dilute acid of gradually augmenting strength. I invariably found that when the acid reached a certain strength violent movements took place, whether the foot only were immersed or the whole legs. This result however is not conclusive, for the even slight movement in the fluid of the small beaker might be considered as sufficient to prevent uniform stimulation of the skin. For exactly the same results were obtained when warm water was applied in the same way as the acid. Instead of the legs remaining quiet, as when the water is still (*i.e.* moved only by the currents of heating), they were withdrawn, with violent movements, when a certain temperature was reached. I further attempted to eliminate the effects of heated blood by ligaturing the legs underneath the sciatic nerves so as to cut off all vascular connection between the legs and the trunk, while leaving the nervous connection intact, and immersing the legs of the animal so disposed in gradually heated water. The diminished irritability however due to the lack of blood-supply and the exposure of the plexus of nerves directly to the vapour and elevated temperature, interfered with the course of events, and I could get no satisfactory results.

Similar attempts were made to obtain analogous results with cold instead of heat; but they resulted in failure. In the first place it was found that (with winter frogs) immersion of the feet directly in water at 0° C. produced no reflex action; *a fortiori* none was produced by the gradual cooling of the water in which the feet were immersed. Only when the feet became actually entangled in the forming ice which spread from the sides of the vessel towards the centre, was any movement visible. Here the stimulus was probably mechanical, due

to traction from the fixed foot, as the animal swung to and fro in the fluid, from the vibrations of the room in which the observation was carried on.

An attempt was also made to use olive oil instead of water; but this failed too, partly from the difficulty of reducing sufficiently and uniformly the temperature of the large body of oil needed for immersion, and partly because the legs were frequently withdrawn when immersed in the oil at the ordinary temperature.

I took the trouble to make these observations because, I am free to confess, I had first leaned to the idea that the chief factor in the matter was the uniform stimulation of a large surface. I called to mind the fact that when we dip one foot into hot water we localize the sensation of heat as most intense in a ring round the ankle marking the level to which the hot water reaches. What we are really conscious of in this case is the contrast between the condition of the surface of the skin in the hot water, and that of the surface outside the water, and this contrast we feel most intensely at the junction of the two surfaces. Normally we are not conscious of the condition of our whole skin when not affected in any particular spot; and yet we have as it were an unrecognized background of such a consciousness with which we compare any local affection. Thus we feel more acutely the temperature of a fluid when we plunge our hand or foot only into it than when we immerse our whole bodies in it. So also it is a matter of common experience that tickling is most effective when the stimulus is applied to a very small surface. The touch of the tip of a feather on the sole of the foot at once produces reflex movements; but the contact of a large pad of cotton wool applied with the same pressure as the feather over a large surface of the sole, may be borne without any uncomfortable sensations, though almost each point of the same surface stimulated separately would at once cause the sensation of tickling. The mutual effect of two neighbouring sensations may also be shewn in the following way. Estimate on any surface of skin by Weber's method the distance at which the sensations of two points merge into one. Then surround the spot of skin so tested with a rim of metal, pressed down with sufficient force to be distinctly felt, but not more.

Test again the power of localisation in the skin within the rim; the distance at which the two points first appear at one will be much increased. Remove the rim and test again. The distance will be found to have returned to its former limit. The simultaneous sensations of the rim have dulled (in some part or other of the nervous mechanism) the sensations arising within the rim. A similar explanation may be given of the fact that it is much more difficult to call forth a reflex action by applying a galvanic stimulus to the nerve-trunk of a frog than by applying the same stimulus to a portion of skin to which some of the fibres of that nerve-trunk are distributed. In the one case a multitude of sensory impulses reaches the cord, in the other a few only, yet movement is absent in the former, though present in the latter. The immersion of the whole leg or of the body in the observations described above may be taken as analogous to the stimulation of the whole nerve-trunk; the immersion of the foot only as corresponding to the stimulation of a portion of skin.

On the other hand, in all observations on the effect of a rise of temperature on living animal tissues, the state of exhaustion or depression which ultimately ensues is preceded by a stage of exaltation in which the functions of the tissue are raised above the normal. This is well shewn in the case of muscles, nerves, and the heart. In none of the observations recorded above was there any indication of such an initiative stage of increased action. Had there been it would naturally have led to the withdrawal of the feet in all cases. And the absence of this presented a great difficulty to considering the results obtained as being merely due to a depression of the powers of the spinal cord by reason of the increased temperature. Some observations, however, made in the Laboratory here by Mr T. O. Harding, afforded a clue, by pointing out a distinction between simply and directly raising the temperature of an organ or a tissue, and indirectly heating it by supplying it with blood heated beyond the normal in some distant part of the economy. Thus the heart of a frog, either empty or filled with serum, when heated beats with a more frequent rhythm and, at first, with greater force. But the same heart when indirectly heated by the immersion of the legs of the frog in hot water (the heart

remaining in the body and the brain and spinal cord being destroyed) is lowered at once both in the force and frequency of its beat, by reason of the heated blood with which it is supplied. This result leads us to expect that in the same way the spinal cord, if heated by being supplied with blood heated beyond the normal, would be depressed without any preceding stage of exaltation, and thus reflex actions which otherwise would have occurred be prevented.

The observation (*Obs.* 7) where the heating one leg prevents reflex action in the other, seems to point distinctly to such an explanation. But the following observation shews still more clearly that, whether or no the stimulation of a large surface may assist in producing the effects described, the main cause is the heating of the spinal cord.

Obs. 8. A brainless frog was so placed in a vessel with a hole in the bottom, that the body and forearms could be exposed to the action of water the temperature of which was gradually raised, while the whole of both legs from the hips downwards hung freely from the vessel, and were not subject to the action of the heated water. Though in an unusual position the frog remained quiet in the absence of stimulation, and executed reflex movements when stimulated so long as the water in the vessel above remained at the ordinary temperature; thus when the toes were made to dip in water gradually warmed, the legs were drawn up after a while as usual. When however the temperature of water in the vessel above, and that in which the toes were dipped below, were both raised *pari passu,* no movements at all took place, and ultimately, as the temperature continued to rise, the body above and the toes below became rigid from rigor caloris (the legs and thighs remaining supple), without any save the slightest spasm. Tested by the dilute sulphuric acid method (which was here practicable) the reflex excitability of the spinal cord diminished as the temperature in the vessel above rose, without any signs of an initiative stage of exaltation.

The depressing effect of a rise of temperature (especially up to or beyond 35° C.) on the energies of the spinal cord of frogs is well shewn in the case of tetanus. Two frogs A and B were each poisoned with a similar small dose of strychnia. To A

4—2

nothing further was done. B, as soon as the spasms manifested themselves, was immersed in water at 37° C.; the tetanic contractions almost immediately disappeared; and the animal when taken out was perfectly flaccid, neither tetanus nor ordinary reflex action being excited by stimulation. After a short while the tetanus returned, and was again removed by a second immersion. This was repeated three times with a like result, the frog A all the while remaining in a state of complete rigidity, and ultimately dying long before B.

We may conclude then that the absence of reflex action in Goltz's experiment, and the other modifications of it, are due primarily and chiefly to the depressing influence of heated blood carried from the skin to the spinal cord. But this depressing influence comes into play by virtue of the gradual character of the stimulation. Dipping a frog either wholly or partially into water of 27° C. or above, at once produces violent movements. When the temperature however is raised gradually the effect on the sensory organs of the skin is much less, and a higher temperature has to be reached before a sensory impulse is generated strong enough to give rise to a reflex action. But by the time that higher temperature is reached the spinal cord has already begun to flag and needs a still stronger impulse, and therefore a still higher temperature in the water acting on the skin; when that still higher temperature is reached the energies of the spinal cord have sunk still lower, and so on stage by stage, until the frog is boiled without having made a sign.

The absence of reflex action with a gradual rise of temperature is still further insured by the stimulus being uniformly applied, *i.e.* by the water being kept as still as possible, and assisted, we may add, by the exposure to the stimulus of a large amount of sensory surface at the same time. Both these circumstances tend to put off the reflex action till a higher temperature is reached, and thus assist in preventing it altogether.

It would be interesting to inquire how far the distinction I have suggested above between directly heating an organ and indirectly heating it by supplying it with blood heated in the extremities is general; and if so, to what changes in the blood the effects are due. They would probably be best shewn in the

frog, whose sensitiveness to elevated temperatures is unfortunately only too well known to physiologists working in the summer. The effects of heating the blood of mammals has already been shewn to be peculiar (see Fick, *Pflüger's Archiv.* v. p. 38).

There remains the question—why does the frog in possession of a brain not behave in the same manner? Why are not his sensations and cerebral processes dulled in the same way by the heating of the blood? The answer simply is, that a less intense sensory impulse is needed to call forth a movement of volition, that is, a movement carried out by the encephalon, than an ordinary reflex action, that is, a movement carried out by the spinal cord alone. The water as it is being warmed suggests a movement to the intelligent frog long before it is able to call forth an unintelligent reflex action. The very first movement of the frog, the removal of any part of his body out of the water, increases the effect of the stimulus; for the return of the limb to the water already warm gives rise to a stronger stimulus than contact with water raised to the same temperature while the limb is still in it; and thus one movement leads to another, and the frog speedily becomes violent. It is nearly the same with the brainless frog, when a movement has for some reason or other been started; only in the observations we have been dealing with this initial movement is wanting.

ON THE LAW WHICH REGULATES THE FREQUENCY OF THE PULSE. By A. H. GARROD, B.A. *Cantab.*

(*Continued from* Vol. VII. p. 219.)

IN a paper on Cardiograph tracings[1] from the human chest wall, in the 5th Vol. of this *Journal*, I have endeavoured to substantiate a law respecting the elements of the heart's beat, which may be thus enunciated :—

The heart's beat consists of two parts, which for any given pulse-rate do not vary in their ratio to one another; but the length of the first part varies inversely as the square root of the rapidity of the pulse.

A second series of measurements of the cardio-arterial intervals, published in the *Proc. of the Royal Society, London,* have further verified the law just stated, and in the rest of this paper it will be assumed as proved. No theory respecting the circulation throws light on its significance; but the one which it has been my endeavour to demonstrate above gives a very satisfactory explanation of it, which will now be considered in detail.

First, the heart's beat consists of two parts, which for any given pulse-rate do not vary in their ratio to one another. It having been proved previously that the pulse-rate does not depend on the blood-pressure, and, as shewn now, the length of the first part of the heart's beat not varying when the pulse-length is constant, it is evident that the length of the first part of the pulse-beat does not depend on the blood-pressure in any way.

Again, the first part of the pulse-beat is compound, for it is the interval between the commencement of the cardiac or ven-

[1] Since writing the paper referred to, a further comparison of tracings has shewn me that in the slow pulses taken while lying, I mistook the primary systolic rise for the auricular, and was so led to the conclusion that the length of the cardiac intervals depended in some measure on the position of the body. This is incorrect, as subsequent measurements shew me, and the length of the first part does not vary with the position of the body; the proper equation for finding the cardiac first part under all circumstances being $xy = 20\sqrt{x}$.

tricular systole and the closure of the semilunar valves; therefore it may be divided into the systole and the valve-closure interval.

Physiologists have laid very little stress on this valve-closure interval, it generally being considered as instantaneous. But in the study of cardiograph tracings it is to be remembered that the distances between events occurring within one-fiftieth of a second of one another can be appreciated without much difficulty, and there is every *a priori* reason for believing that this interval has a longer duration than that. In my paper on the Cardiograph trace, reasons have been given for the belief that in quick pulses the commencement and the end of this valve-closure interval are indicated by separate and distinct changes of direction in the curve, and its length as obtained by measuring from these points agrees entirely with that required from arguments to be mentioned further on. It may be called the *diaspasis,* that is, the period during which the heart is being opened out by the regurgitation of blood from the arteries.

The length of the combined systole and diaspasis not depending at all on the pressure, and it being constant for any pulse-rate, it is infinitely probable that the systole and diaspasis separately are independent of the pressure; and this is extremely interesting, as it gives a further insight into the mechanism of the heart. For, in order that the duration of the diaspasis should not vary with different blood-pressures, it is evident that with higher pressures there must be greater obstruction to the heartward flow of blood, otherwise the valves would then close more quickly. And this is exactly what would be expected from the combination of Mr Bryan's observations concerning the shape of the heart, and Brücke's theory of the active diastole of the ventricles[1]. According to the latter author the cardiac muscular tissue has no inherent power of opening out the ventricles, but remains inactive after systole, during diaspasis in fact, until the regurgitation from the aorta

[1] Mr Bryan's paper is in the *Lancet*, Feb. 8th, 1834.
Brücke's theory appeared in *Sitzungsberichte der Wiener Akad. der Wiss.* 1854, Vol. xiv. p. 345. See also a paper on the same subject by myself in this *Journal*. May, 1869.

has closed the aortic valves and so uncovered the orifices of the coronary arteries, immediately upon which, the resulting sudden turgescence of the heart's walls makes them open up. Mr Bryan has shewn that during systole the whole heart alters its position as a result of its change in shape during contraction, and recovers it during diastole; therefore the greater the force of contraction the more will it alter its shape, and the more difficult will it be for it to resume the original one, which has to be done partly by the regurgitating arterial blood; but the greater the blood-pressure, the greater will be the facility for overcoming this greater work, which two, as must be the case, vary together. This argument explains how the diaspasis need not vary in length with different blood-pressures.

Next, with regard to the systole. As the first part of the heart's beat varies as the square root of the length of the beat, and as the diaspasis, a part of that first part, does not vary with the blood-pressure, upon which, and the rapidity of tension fall (which must be a comparatively insignificant force), it can alone depend, it is necessary that the other component of that interval must vary more than as the square root of the pulse-length. And to find how much more quickly, it is necessary to obtain the actual length of diaspasis in a particular case, from which a close approximation can be arrived at as to its duration at all rapidities. Careful measurements of a cardiograph trace, beating 102 in a minute, give the ratio of the systole to the whole beat as 1 to 3·1915, and that of the first part to the whole beat as 1 to 2·0, which leaves the ratio of the diaspasis to the beat as ·187 to 1, or the diaspasis length as ·00183 of a minute. A very similar length of diaspasis is found from quicker pulses.

The great interest attaching to these figures is that, when with the diaspasis equal to ·00183 of a minute, the ratio of the systole to the diastole is enquired into, it is found that there is a very simple relation between them, and that after subtracting this diaspasis of nearly constant length, there remains the systolic varying as the square root of the diastolic period, and with no other diastolic length is so simple a ratio obtainable, which is all-important, because it will be seen that the systole must depend directly on the previous diastole.

Next, considering the systole itself; the fact above demonstrated, that its length does not depend on the blood-pressure is extremely important, and can only be explained by assuming that when the pressure rises, the circulation through the coronary vessels increases to a sufficient extent to enable the heart to get through the extra work it has to perform without altering the duration of its action, or in more precise terms, the nutrition of the walls of the heart must vary directly as the blood-pressure in the aorta.

But the systolic length varies as the square root of the diastolic if the argument which is developed above is correct, in other words, the longer the time of nutrition of the heart, the longer is the systole. This at first sight seems an anomaly, but the theory that the pulse-rate depends on the fall in tension only, presents a most complete explanation, and so throws great light on cardiac action in general.

Consider the heart as a pump working against a certain pressure, and filling an elastic reservoir with a certain resistance to the outflow of its contents. Varying the pressure has been shewn to have no effect on the lengths of the different parts of the pulsation for the reasons given above; and it has next to be considered how it is that varying the resistance changes the lengths of the elements of the revolution. This pump, directly its muscular fibres begin to contract, exerts its full pressure, for there is nothing to prevent it doing so. But during the previous diastole it was supplied by blood at a certain definite pressure and for a definite time, both of which factors limit the force of the systole. Consequently the ventricles produce directly their full systolic pressure, and maintain that pressure until they are empty. But it is evident that the time necessary for emptying them of a definite amount of blood under these conditions must depend on the rapidity of the flow from the capillaries, for when the flow is halved the systolic time must be doubled, if no other force come into play; in other words, the length of cardiac systole is a function of the arterial resistance; and the pulse-rate has also been shewn to be a function of the same, upon the fall of tension theory.

It has been proved that the systole varies as the square root of the diastole, not directly with it, as might be supposed.

This clearly shews that the time of diastole influences the length of the systole and shortens it, in other words, strengthens the heart, according to the law which may be stated thus, the nutrition of the heart varies as the square root of the time during which the coronary circulation is maintained.

It will strike some as peculiar that no mention has yet been made of the influence of the nervous system on the heart. But it appears to me that the facts which have been brought forward have not called for any special reference to it. May not the law it has been my endeavour to prove, be but an expression of that action in the healthy body? For it must depend on a somewhat complicated mechanism, as is shewn by the fact that it is almost impossible to contrive a self-acting engine which would pulsate in accordance with its requirements.

As is well known, the effect on the kymographion trace, of slightly stimulating the pneumogastric nerves, is greatly to amplify the oscillations, and at the same time to lower the mean pressure; while cutting them produces the reverse effects. The larger oscillations of the hæmadynamometer column in the former case, shew that the proportionate tension-fall and the time of pulsation are both greatly increased, and from previous considerations it is evident that these are necessarily associated, when as now, no influence is being exerted on the peripheral vessels[1].

Further, these amplified oscillations must be attended with an abnormal enlargement of the ventricular cavities during diastole, for the time intervening between the beats being increased, the amount of blood which flows through all segments of the circulation between any two pulsations, must be also more considerable. Having arrived so far, it is extremely interesting to observe how an augmentation in the degree of cardiac dilatation during diastole, as a cause, will include and correlate all the peculiarities which are observed when the pneumogastric nerve is thus operated on; and it is not unreasonable therefore to suppose that this is the direct effect of its action. As the quantity of blood contained in the heart at the end

[1] In rabbits the normal fall of tension as judged by the hæmadynamometer trace is about $\frac{1}{20}$th of the whole, while when the pneumogastric is stimulated it may increase to $\frac{1}{10}$th or more.

of diastole has been shewn to depend on the circulation through the coronary vessels, it is evident that the explanation of any variations in the capacity of the ventricles must be referred to changes in the cardiac walls themselves. Just as the degree of rigidity of an india-rubber tube through which a current of water is flowing, can be made to vary by changing the diameter of the orifice from which the fluid is allowed to escape, so the turgescence of the ventricular walls, or what is the same thing, the amount of active diastole of the heart, can be altered, by varying the diameter of the small arteries of the coronary system, their contraction producing a greater, and their dilatation, by facilitating the flow of blood through the capillaries, a less degree of diastolic enlargement of the ventricular cavities.

From the above argument, therefore, the amplified range of pressure and time depending on change in heart-capacity, and the change in capacity being caused by modification in the calibre of the smaller coronary arteries, it is almost a logical necessity that the function of the pneumogastric nerve is to regulate the degree of tonicity of those vessels, and Dr Brown-Séquard, from entirely different facts, has also published it as his belief that the pneumogastrics contain fibres which contract the small coronary vessels[1].

It will be noticed that throughout this paper it has been assumed that the systole never recommences until the ventricular cavities are completely filled, that is, until a pressure equilibrium has been arrived at in the interior of the heart. Perhaps it is the absence of pressure which admits of the heart recontracting, but this is a doubtful point, and until more is known as to the mechanism of muscular action in general, it is probable that the question as to the reason why the heart recommences to beat at a particular moment will remain unsettled. Sir J. Paget[2], when he pointed out the relation of rhythmic nutrition to rhythmic action of nerves and muscles, laid the foundation for a scientific treatment of the subject, and the law which it has been my endeavour to substantiate, is only a precise method of expressing that relation.

[1] See *Principles of Human Physiology*. By Dr. Carpenter, 1869, p. 219, foot-note.

[2] *Croonian Lecture*. Royal Society, 1857.

The following summary of the main features in the circulation, as they appear to me, may assist in explaining some of the previous arguments.

The circulation of the blood is maintained by the repeated contraction of the heart. Each cardiac revolution is divided into three parts, the systole, the diaspasis, and the diastole. The following laws hold with regard to the length of these intervals.

I. The systole together with the diaspasis, or in other words, the first cardiac interval varies as the square root of the whole revolution.

II. The systole varies as the square root of the diastole.

III. The diaspasis varies very slightly with different pulse-rates.

The amount of work that the heart has to perform in maintaining the circulation depends on two sets of changes which may occur in the system; 1. Variations in the blood-pressure. 2. Variations in the resistance to the outflow of that fluid from the arteries.

As the capacity of the arteries including the ventricles, varies directly as the blood-pressure, and as the flow of blood from the capillaries does the same, the frequency of the heart's beats is dependent on the resistance to the capillary outflow, and not at all on the blood-pressure; in other words, the heart always recommences to beat when the blood-pressure in the systemic arteries has fallen a certain invariable proportion.

Variations in blood-pressure result from: 1. Absorption into, and excretion from, the vascular system, of fluids. 2. Changes in the capacity of the arterial system, which occur on the contraction or relaxation of the muscular arteries. 3. Changes in the amount of available blood, which result from the hæmastatic dilatation of some of the yielding vessels on altering the position of the body. As changes in the first of these cannot be very sudden, and those in the latter are never very considerable, the mean blood-pressure in health varies but little during short intervals.

Variations in peripheral resistance result from: 1. Different degrees of tonicity or patency of the muscular arteries. 2. Dif-

ferent resistances in the venous system. The former may occur independently in one or other system of vessels, as the cutaneous or the alimentary; also mechanically from pressure on a part of the body. The latter are insignificant in health.

The heart depends for its power of doing work on chemical properties in the blood it pumps into the systemic vessels, and as the blood reaches it direct from those vessels, the cardiac intramural circulation varies with the changes in the former; and the length of the systole varying only as the square root of the time of diastole, the degree of cardiac nutrition varies directly as the systemic blood-pressure, and as the square root of the diastolic time. The coronary arteries supplying the whole heart, the work done by the right ventricle is governed by that done in the left; thus the supply of blood in the left auricle is always rendered sufficient for the requirements of the systemic circulation; though, as there is no reason for believing that the resistance in the pulmonary vessels varies with that of the systemic, there must be some peculiarities in the former circulation (which may explain the variations in the ratio of the number of pulse-beats to respirations, in some cases).

The auricular contraction is a very small force, and its function is most probably to close the tricuspid and mitral valve.

The heart commencing its systole as a whole, it is highly probable that the impulse for action is given by a force which affects both ventricles; such is found in the coronary circulation and the active diastole produced by means of it.

In conclusion I have to present my best thanks to Dr Michael Foster, Professor Sanderson, and Professor Pritchard of the Royal Veterinary College, for the opportunities they have afforded me in trying the experiments above detailed; without their assistance it would have been impossible for me to have put the law of the relation of blood-pressure to pulse frequency on any satisfactory basis.

MORPHOLOGICAL ELEMENTS OF THE SKULL.

By W. Kitchen Parker, F.R.S. (Pl. IV.)

The skull, or cranio-facial skeleton, would be easily comprehended as a morphological structure, were it no more complex than the skeleton of the trunk.

In that part of the vertebrated animal we have merely a repetition of essentially similar segments, each, typically, forming a *ring* above and a *hoop* below. Over these hoops at two points at the most there are the paired fins or limbs, with their imbedded and free portion—limb-girdles and limbs proper—and in fishes the unpaired upper and lower fin-rays, with their imbedded cartilaginous or osseo-cartilaginous roots.

But the essential, primary parts of the axial skeleton have a totally different "habit" in the region of the head, to what they have along the spine; the notochord, truly, is there, and the substance which invests it ("investing mass"), but this never shews the least tendency to break up into "somatomes," and "metasomatomes," as in the body. Moreover, although the cranium contains a continuation of the common neural axis, yet in this region, instead of a simple nervous tube, *grey* within, and *white* without, in the head the nervous mass always[1] swells into three principal vesicles, which are grey *without*, and white *within*.

Also let it be noted that it is only the medulla oblongata, an evident continuation of the spinal chord, and the cerebellum which are enclosed in the occipital ring, or that part of the cranium which is constructed out of the fore end of the notochord, and its undivided investing mass.

The box, then, in which the cranial vesicles and their often immense and very complex outgrowths are enclosed, is a *compound* structure. Before it is treated of it will be necessary to describe and to classify the other skeletal parts of the head.

[1] I purposely omit the Lancelet (*Amphioxus*), from all consideration, at present; I begin with the Suctorial Fishes ("Myxinoidei," "Marsipobranchii"). We get no proper series that can be considered through and through until we come to the Lamprey and its relations.

I believe that the only truly cartilaginous parts of the *Amphioxus* or Lancelet are the "palpi" or "cirrhi:" these *proto-skeletal* parts re-appear in the higher fishes, and in some of the air-breathing types.

Then besides these we have in the suctorial fishes a sub-cutaneous system of cartilages, immensely developed in the lips ("labial cartilages"), and in the pharynx; forming in the Lamprey the beautiful branchial basket.

In these fishes, besides the outer cartilages, which I propose to call "extra-visceral," there is a cartilaginous skull, and also *three pairs* of what are called "visceral arches" by embryologists. One pair of these bars is in front of the mouth, and two pairs behind; there is also the rudiment of a second præ-oral pair; in embryological works, the first post-oral, or mandibular arch, is called the first visceral arch.

These "Marsipobranchs" also possess "sense-capsules," which more or less modify the form of the cranium, the last pair, or auditory sacs, being compacted into the posterior part of the cranial side-wall.

In the next group of fishes, the Sharks and Rays, or "Elasmobranchii," the cartilage, which is soft in the "Suctorii," becomes calcified externally, and sometimes within; but the ossifications that occur in the cutaneous system with its aponeurotic enfoldings and its various strata, looser or more dense, do not become modified correlatively to the cartilaginous skeleton. This first shews itself as a new morphological specialization in the "Ganoid" fishes.

The elements that go to form the skeletal parts of the head of a vertebrate, some height up in the scale, are as follows:

a. Cirrhi.
b. Extra-visceral cartilages.
c. Sub-cutaneous bony plates.
d. Visceral cartilages (arches).
e. Sense-capsules (mostly cartilaginous).
f. Cranium, proper; composed of

1. Notochord and undivided part of "investing mass."

2. General membranous coat, which is chondrified below and behind by the "investing mass;" below and above by the arch formed from that mass (occipital arch); and anteriorly by an independent growth of cartilage.

3. Bony plates developed in subcutaneous tissue *outside* the chondrified *external* layer of the membranous cranium, such as the frontals and parietals.

But the eye-balls are the only pair of sense-capsules that grow free of the cranial walls—they indeed modify the shape of the box, being nested in its sides. The other sense-organs are most intimately built up into the very structure of the cranium.

This is not all, for certain segments that appear in the adult cranium in the higher types, notably in the mammalia, are formed by ossification of *compound* cartilaginous tracts, formed by early fusion of the first præ-oral (*actual first*) visceral arches.

It is also to be remarked that the more the brain develops—as we ascend among the types—the less does it owe to the primordial cartilaginous thickening of the outer stratum of its membranous investment. The inner stratum ("dura mater") always retains a fibrous character.

So also as to the visceral arches: these are huge in fishes, especially when they are only partially calcified, as in Sharks and Rays; but as we ascend they become more and more liable to histological metamorphosis; becoming arrested, and often being replaced, functionally, by subcutaneous bones, or by these together, with "extra-visceral" cartilages.

An instance of this is at hand in the Pig—which undoubtedly represents a large group of mammals—where the primary mandibular bar is arrested above, absorbed below, and gives place functionally to the combination of a labial cartilage with a subcutaneous ossification.

Both in the nasal sacs and in the organs of hearing the true cartilaginous investment of the sense-organ itself forms metamorphic combinations with contiguous visceral arches.

With regard to the visceral arches if we examine a series from the Chondrosteous Ganoids (Sturgeon, &c.) upwards, we shall find a very large amount of variation in the metamorphosis of their substance. *Below*, a thin ectosteal plate may invest a permanent core of unchanged hyaline cartilage; but *above*, as in the hot-blooded classes, the arches may ossify, whilst as yet their substance is a soft granular mass of "indifferent tissue."

Not only so, the arch, originally visible as a solidish granicular tract, may chondrify here and there; large tracts being arrested, and becoming lost in the general fibrous stroma amongst which it was developing.

Segmentation of the "visceral arches."

The morphological observer notes especially in the skull how it is compacted together by that which each element supplies to it,—for we have just seen that this important box is formed of very diverse parts and structures.

But whilst all tends to solidity in the *cranium*, in the face, in many instances, the parts are specialized in relation to the most varied movements: hence the large amount of segmentation seen in the visceral bars.

The first pair of visceral bars, the "trabeculæ cranii" of Rathke, are least mobile, and therefore least segmented; the most mobile are the parts that lie around the mouth and throat.

Before describing the very uniform manner in which, on the whole, the visceral arches break up, it may be well to remind the reader that they must never be confounded with the costal arches that spring from the bodies of the vertebræ. These *oro-faucial* bars are developed about the mouth and throat, at first as thickenings, whilst the face of the embryo is growing into a ridge-and-furrow structure: the ridges develop a *pith*, which is the visceral arch, and the furrows are the first sign of that *dehiscence* which takes place to form the "visceral clefts."

These visceral clefts are the doors and windows through which the vertebrate creature keeps up its necessary commerce with the surrounding world for food, and drink, and

air. As a rule the clefts are in distinct pairs, but the pair which form the mouth run into each other at the ventral line.

The metamorphic changes of the visceral arches rise, as it were, one above another in the scale of life, the higher type having "nothing over," no more change than its habits demand; and that which undergoes but little modification has "no lack;" for though its parts suffer less change, yet they answer to the necessities of the creature as perfectly as in the nobler forms.

We have seen that the true visceral arches are few in number in the "Suctorial Fishes;" but amongst the "Placoids" or Sharks and Rays there are, normally, five pairs of "branchial arches" behind the hyoid or arch of the tongue. In the embryo of the shark (*e.g. Scyllium canicula*) both the mandibular and hyoidean arches carry caducous (external) gills, and the hyoid carries a *half-gill* in the Placoids, generally, and in most of the "Ganoids;" yet in the Teleostean or Osseous Fishes the gills are confined to the arches behind the hyoid. The last arch is abranchiate, and in Osseous Fishes forms the "nether millstone" to the grinding apparatus, the upper stones of which are formed by the apical segment of the gill-arches in front of it: the papillæ on that, the "pharyngo-branchial" segment, becoming developed into teeth, and not into gill-plates. That the arches in front of the mouth are the equivalents of those behind is shewn by the development of the mouth itself, and also by the presence of a *præ-oral cleft*, the passage which in us conveys the tears into the nose by the lachrymal duct.

Normally, in Fishes above the Suctorii there are seven postoral arches: in the Anourous Amphibia only six. In the Shark and its congeners I find no pharyngo-palatine or second præ-oral arch; and for the time of the greater part of the tadpole-life of the frog this arch is suppressed, or nearly so, and it is never a distinct bar. I am satisfied that it is also suppressed permanently in the Tailed Amphibia, and that it is only a rudiment, not free, in the Suctorii. These things are not, however, all quite certain at present.

As a rule the segmentation of the visceral arches takes place some time anterior to ossification, that is in the Gill-bearing, or Ichthyopsidan types: in the higher forms, in some

of the arches, at least mostly those in front of the mouth, the osseous deposits determine the segmentation, and now and then in Birds a single bony part breaks up into two separate bones.

In the "Branchiata" segmentation is generally greatest in the hyoidean and branchial arches. In the latter it may be said to be typical; but the hyoid arch of a Shark is arrested as to its subdivisions. That arch in the Teleostei runs into parts beyond what is typical.

Furthermore, whilst in the Branchiata the whole seven of the post-oral arches become converted into hyaline cartilage, at least, if not bone, in the Abranchiata only the first three of these are developed, the last of the three only partially.

The *form* of each arch is sigmoid, much like the italic letter *f*, they generally turn both apex and distal end inwards and backwards; but sometimes the direction of the apex is forwards, at others simply inwards. The arches about the mouth have a very regular habit of grafting themselves upon other parts, parts having a diverse morphological origin; for instance, the trabeculæ thus seize upon the apices of the "investing mass," the pterygopalatine seeks the fore edge of the mandibular pier, and that pier, the apex of the first post-oral arch, whilst the hyoid grows to and grafts itself upon the auditory capsule. As a rule, the proper branchial arches turn their apices over the pharynx; and in the Shark, especially, it may be seen that by stretching or contracting that large tube, these cartilages that enring it may be turned inwards and forwards from their normal backward direction.

Passing suddenly from one type to another in my *slow* researches—a year on an average to a type is not an unusual period, so that I may say ten years have I consumed over as many skulls—I became conscious that the same essential thing was still before my eyes. Not troubling myself with the motto, "Nihil per saltum," but passing at once from the Pig to the Shark, I became conscious of a nearness in nature of these two most diverse subjects, that was not a little startling.

Filling-in this space, and looking this way and that for light, it has been gradually revealed to me that there is a marvellous uniformity in *number*, in the segments of a visceral

arch, wherever found. At least the *same number* will turn up so frequently that there can be no doubt that there is some law to be discovered causing this uniformity. Not to lay too much stress upon this subject, it yet appears to me to promise as much fruit in morphology as the number of members in the whorls of a flower in various natural orders—three, four, or five, as the case may be. Running the eye over the branchial arches of those three great groups of Fishes called "Elasmobranchii," "Ganoidei," and "Teleostei," we shall find that Professor Owen's elegant nomenclature for these parts is applicable throughout, namely, from above downwards the segments are known as "pharyngo-" "epi-" "cerato-" "hypo-" and "basi-branchial"—the latter element uniting the right and left moieties of the arch (fig. 9).

The same prefixes may be made applicable to the segments and *regions* of all the visceral arches; and they may serve for the *generic* as well as *specific* names of these arches.

Thus for the whole genus, "visceral arch," we have segments or regions that may be called, throughout the series of arches and in all the types of the vertebrata, "pharyngo-," "epi-" "cerato-" "hypo-" and "basi-visceral." These prefixes can as well also be made to fit on to the fore-front of such *specific* terms as "trabecular," "palatal," "mandibular," and "hyoidean," as well as "branchial."

To apply this, the main segments in the much-divided hyoid arch of a mammal are as follows: namely, at the apex the "os orbiculare" next below the "incus;" then the "stylo-hyal," below this the "cerato-hyal," and, uniting the two sides, the "basi-hyal" (fig. 9). These all exactly correspond with the typical segments of those well-developed visceral arches, the "branchials" of the three largest orders of Fishes—the groups in which these visceral arches have their fullest development, and are most typical, morphologically. It may be objected that there is an additional piece in the hyoid arch of a mammal that I have not mentioned, namely the little "inter-hyal" segment which gets attached to the neck of the "stapes," or auditory plug.

There is this additional piece, and in the hyoid arch of the osseous Fish it is also present, but it is a *secondary* morpho-

logical element, and is of the same value in morphology as a "meniscus" between two segments. The hyoid arch of the osseous Fish also goes beyond the typical visceral arch by having two centres of ossification for each of three of its segments, namely for the so-called hyo-mandibular, the main bar, and the distal piece or "hypo-hyal."

This tendency to further segmentation by the superaddition of fresh osseous centres, is conjoined in these fishes with the absence of the apical piece, which is so well developed in the branchial arches as the "pharyngo-branchial" segment.

For a long while the meaning of the hyoid arch of the Frog, after metamorphosis, was hidden from me, for, besides the chief segments of the osseous Fish, the Frog, and its congeners, the other "Anoura," have an additional segment above the hyomandibular (= *incus*); which I saw long ago must be the counterpart of our little "os orbiculare," wedged as it is between the incus and stapes. This part, therefore, seemed to be a *new thing*, a specialization not to be accounted for by any normal segmentation of visceral arches: it is, however, merely a "pharyngo-hyal" segment, and is perfectly normal.

The manner in which the "cornu-minor" of the mammalian "os hyoides,"—the hypohyal segment—turns backwards to articulate with the basal piece, is also quite normal (fig. 9); in like manner the *hypo-trabecular* region of the first arch is always hooked backwards, and the *two diverging* hooks are the "trabecular horns" (fig. 10). The manner in which the right and left moieties of these arches join each other, and the mode of junction of a bar with one in front of, or behind it, is very interesting.

The junction of the right and left parts of the same arch at the mid-line below may be called the visceral "commissure," the yoking on of arch to arch a visceral "conjugation," and such projections or parts as are developed may be termed conjugational processes. The "basi-pterygoid processes" of the trabeculæ, and the "orbital processes" of the palatals, are conjugational processes: the "basi-visceral" elements are specializations of "visceral commissures."

Now the azygous, or commissural pieces, have been long known in the post-oral arches, as in the basi-hyal, and basi-

branchials, but basal elements have not hitherto been described in the præ-oral arches. In various papers on the bird's skull I have made mention of a "præ-nasal rostrum," a cartilage that projects from the fore-end of the nasal septum, and is the model on which the very prognathous præ-maxillaries of the bird are formed. In the paper on the fowl's skull (*Phil. Trans.* 1869, Plates 81—87), the reader will find the genesis of this cartilage, and it is one of great interest: it is, I now see, the "basi-trabecular" element (figs. 5—8). I am now familiar with this part in rays, sharks, serpents, tortoises, birds, and mammals: the degree of modification and development this bar undergoes determines very largely the form of the face. In my first plate of the fowl's skull (Plate 81, fig. 3, beginning of 2d stage) this cartilage (*p. n.*) is a rounded bud, looking downwards and backwards. This is similar to what I have lately described in the embryo pig, 7½ lines in length; in this latter animal it is arrested at this stage, and the præ-maxillaries are formed *beneath* it. In the turtle, *Chelone midas*, it straightens down, and the præ-maxillaries are formed in front of it. In birds (as in sharks and rays) it straightens still further (*Op. cit.*, Plate 81, fig. 4, Plate 82, figs. 1 and 2, and Plate 83, figs. 1, 2, 4, 5, *p. n.*), and projecting in front of the face forms a most fit model for the præ-maxillary *parostoses* (figs. 6—8). The least change in the direction of these two bones, whilst still growing upon the *transitory* cartilage, makes all the difference between the beak of a crow, a hawk, a hornbill, or indeed any type of this beaked class. In the snake (fig. 3), as in the mammal, it is arrested, and keeps the backward position it has in common with the "hypo-trabecular horns." It was a satisfaction to find a morphological "pigeon-hole" for the præ-nasal cartilage, and at the same time a basal element for the newly-discovered trabecular visceral bars[1]; but my labours have

[1] I had long satisfied myself that the old term *inter*-maxillary was better than Professor Owen's new name *præ*maxillary, and I had stated this to Professor Huxley, for I saw that the base of the nasal septum was with its "præ-nasal rostrum" the axis or primordial part of the foremost præ-oral arch. But it was Professor Huxley who shewed that the whole of each trabecular bar belonged to this part; it was suggested to his mind by the manner in which the nasal diverticulum of the Lamprey passes between the ethmo-vomerine cartilage (cornua trabeculæ) and the skull-bone. But more than thirty years ago Johann Müller (in the Chapter on Generation in his *Physiology of Man*), shewed

been further cheered by the unveiling of a like part in the very variable pterygo-palatine bar.

I have already stated that the second præ-oral arch is ossified in the "Abranchiata" whilst quite soft. This is true on the whole; but I had long ago noticed that in the higher birds certain parts of it were slow to ossify, and were developed into hyaline cartilage first. It then turned up that in the woodpeckers ("Picidæ") the contiguous edges of the palatines became cartilaginous, and that this "commissure" was afterwards formed into a dagger-shaped bone—a "basi-palatine." The same thing takes place in *Caprimulgus europæus;* it has two bones in this part, and so also has *Podargus;* and I expect to find it in many others[1].

The "hypo-palatine" region is developed in the Salmon into a large knob of cartilage in front of the trabecular conjugation.

In no creature have I yet seen a distinct "pharyngo-palatine" element in a separate state; but in most of the Lacertians the "epi-palatine" is more slowly ossified than the rest of the arch, which bifurcates *behind,* the upper becoming a sub-cartilaginous rod; this rod, ossifying, becomes the so-called "columella"—my "epi-pterygoid." This element also exists in the Chelonians, and becomes flattened and wedged-in between the descending parietal wall and the floor-shaped "pterygoid." In birds this part is shewn as a "process," and is a very elegant hook in Passerine birds, and in such forms as *Scythrops*, *Podargus, Megalæma.* In mammals also it is a notable part, most elegant in *Tragulus javanicus,* but known long ago in man as the "hamular process of the internal pterygoid plate."

These instances of parts shewn to be classifiable must suffice at present; they can easily be referred to by the student, and I flatter myself that they will commend themselves to his judgment.

An attempt to harmonize the "labials" and outer branchial cartilages as "extra-viscerals," will, perhaps, be made at further leisure; but it may not be thought out of place if I remark

that the inter-maxillary apparatus was developed independently of the palato-maxillary.

[1] A supplementary paper to the one just quoted on the Fowl is ready for publication, in which these structures will be illustrated and described.

upon the relation of the cephalic skeleton to that of the trunk.

In the present state of our knowledge it is better for us to consider the morphology of the skull as quite distinct from that of the body.

Firstly. The formation of somatomes in that part of the "mesoblast," which lies on each side of the notochord, takes place very early, and is never seen in the cephalic region.

Secondly. The pituitary body, growing downwards and backwards from the first cerebral vesicle, beneath the second, effectually stops the notochord in its forward growth: the "inverting mass" ends in a pair of blunt, or even squared extremities, opposite the end of the notochord.

Thirdly. The three well-known basal pieces of the skull, which beguiled the older observers, who saw, or thought they saw, in them three more vertebral centrums, are very diverse, morphologically, and only the last contains any of the notochord.

Fourthly. Ribs are direct down-growths from the sides of each vertebral "centrum:" nothing of the kind is developed in the head, where the basi-occipital represents several *potential* centrums.

Fifthly. Any arches in the head and embracing the throat-region, which should correspond to ribs, would enclose the heart: the visceral arches would lie *inside* such hoops, and the heart lie outside (below) the visceral arches.

Sixthly. The "extra-visceral" bars, outside the branchial arches of the shark, which lie close under the skin, like ribs, have no similarity in their origin to those arches of the trunk-vertebræ.

EXPLANATION OF PLATE.

Magnified figures of certain visceral arches of Vertebrata.

1. Side view of third branchial arch of adult dog-fish (*scyllium canicula*), *p. br.* pharyngo-branchial; *e. br.* epi-branchial; *c. br.* cerato-branchial; *h. br.* hypo-branchial; *b. br.* basi-branchial; *br. r.* branchial rays; *ex. br.* extra-branchial.

2. Under view of snout of *the same; tr.* trabeculæ; *c. tr.* cornua-trabeculæ; *b. tr.* basi-trabecular rostrum; *l.* labials.

3. Under view of snout of embryo snake (*natrix torquata*); letters as above.

4. Side view of the same.

5. Side view of snout of passerine bird ("Avis Ægithognatha"). 1*st Stage.*

6. Ditto " " Ditto 2*nd Stage.*

7. Ditto " " Ditto 3*rd Stage.*

8. The same as last; under view.

9. Hyoid arch of embryo pig (*sus scrofa*); *o. o.* os-orbiculare (pharyngo-hyal); *i.* incus (epi-hyal); *st. h.* stylo-hyal (cerato-hyal); *c. m.* cornu minor (hypo-hyal); *b. h.* basi-hyal; *i. h.* inter-hyal.

10. Side view of snout of embryo pig: *lettering* as in figs. 3—8.

ON DOUBLE NERVE STIMULATION. By A. G. DEW-SMITH, B.A., *Trinity College, Cambridge.* Pl. V.

(From the Physiological Laboratory in the University of Cambridge.)

THE problem, the solution of which I have attempted, and which, as far as I know, has not hitherto engaged the attention of any observer, is as follows:

If two pairs of electrodes be placed on a nerve, one pair being nearer the muscle than the other, what happens when a stimulus is sent through both pairs of electrodes at the same time? Taking the contraction of the muscle supplied by the nerve as a measure of what is going on in the nerve, is the contraction which results from a stimulus thrown through either pair of electrodes singly at all influenced, and if so in what manner, by the simultaneous stimulation of the nerve at the other pair of electrodes?

The first observations were naturally made with a single induction shock and with the opening or breaking shock. It seemed desirable also to begin at least with an equal, and that a weak stimulus, or one below the maximum, at the two pairs of electrodes. But here four different cases presented themselves, according to the direction of the current; these cases would need to be distinguished, as the results might be different in the different cases. Calling the electrodes farther away from the muscle *A*, and those nearer the muscle *B*, we have:

Case I.	where the current in	*A* is descending,	in	*B* descending,
... II.	......	*A*	......	*B* ascending,
... III.	......	*A* is ascending,	in	*B* descending,
... IV.	......	*A*	......	*B* ascending.

In Case I. and IV. the currents have the same direction, in II. and III. opposite directions.

The majority of my observations were made on Case I.; the other cases were examined afterwards for the purpose of ascertaining any differences from that case. I first attempted to use a divided circuit, interposing both the *A* and *B* electrodes in

the circuit of the secondary coil. The electrodes used were platinum wires fixed in paraffin blocks; the distance between the anode and kathode of each pair (about 3 mm.) was thus maintained perfectly uniform. An observation was conducted as follows. The sciatic nerve of a frog was placed on the *B* electrodes alone, the upper part of the nerve resting on the clean paraffin block. On the electrodes *A* was placed a portion of the other sciatic nerve of the same frog, carefully selected, so as to correspond as exactly as possible to that portion of the nerve which would lie on the electrodes *A* when both *A* and *B* had to be stimulated at the same time. The gastrocnemius muscle belonging to the nerve was attached to a simple lever writing on a smoked surface, the excursions of the muscle being magnified several-fold, in order to be able to use only a very slight stimulus, and yet measure successfully the variations which occurred. It was hoped that in this way the resistance at the electrodes *A* might be maintained the same, both when *B* only was stimulated, and when *A* and *B* were stimulated together, and that consequently the amount of stimulus reaching *B* would be the same, whether the nerve were stimulated at the same time at *A* or not. Similarly, when it was desired to stimulate *A* alone, a piece of the nerve of the other leg, corresponding to the piece which would lie on *B* when the nerve was placed on both electrodes, was carefully laid on *B*.

After a few observations, however, it was soon evident that this method was impracticable. However carefully done, the shifting of the nerve on *A* caused marked variations in the resistance at the electrodes *A*, and consequently variations in the amount of stimulus sent into *B*.

An attempt so to increase the total amount of resistance in the circuit that variations at *A* might be neglected, introduced other elements of confusion, and I was consequently led to make use of two separate and independent induction machines, with independent primary circuits, the secondary coil of each machine being connected, of course, with its own electrodes alone. By means of two pairs of mercury cups, the two primary circuits could be made or broken, singly or together, at pleasure; or by means of a key either secondary circuit could be short circuited off from the electrodes. By either method *A*

and B might be stimulated together, or either of them apart. The two machines were placed at right angles to, and at some distance from, each other; and by placing a separate nerve and muscle on each pair of electrodes, I satisfied myself that the two machines had no influence on each other.

In this way I made a large number of experiments, without however arriving at a result which I could consider thoroughly satisfactory. For instance, when the contraction resulting from the stimulation of A was about equal to that resulting from the stimulation of B, the contraction given by A and B together was in the majority of cases equal to that of B alone. A small number of exceptions, however, always occurred, in which the contraction of $A+B$ was greater than B. Upon consideration it seemed probable that the cause of these exceptions was to be sought for in the want of exact simultaneity in the breaking of the primary circuits of A and B, and consequently in the stimulus arriving at the one pair of electrodes a small fraction of a second sooner or later than at the other. That this cause was the true one became evident when, instead of employing an ordinary rotating cylinder for registering the contractions, I began to make use of the pendulum myographion.

In as much as the instrument in the Laboratory was fitted with a single contact-breaker only, I at first so arranged matters that the pendulum in its swing broke an independent current, which, by means of a steel slip and an electro-magnet, was keeping two platinum points dipping in two cups of mercury. At the breaking of this current the steel was released from the electro-magnet, the two platinum points raised from the mercury cups, and, consequently, the circuits of which they respectively formed part broken. By raising (or lowering) one or other of the platinum points, the moment at which contact was broken in the one cup was thrown in front or behind that in the other.

This method, however, proved on trial inexact, and I consequently replaced the original single contact of the myographion by a double contact-breaker, similar to the one employed by Helmholtz, so arranged that the one contact-breaker could be shifted in front of or behind the other, and thus the catch of the pendulum would break the two contacts

at an interval of time, varying according to the distance through which one had been moved.

My results then became for the first time satisfactorily uniform. When contact is broken in both the *A* and *B* circuits simultaneously there is no increase of attraction; the resulting curve is the same as if the nerve had been stimulated at *B* only. Thus, in Fig. 1, which reproduces a photograph taken from the myographion plate, 1 represents the muscle-curve resulting from the stimulation of *A* alone, with a single opening induction shock derived from a single Daniell with the secondary coil of a Du Bois Reymond's machine at 25. 2 is in like manner the muscle-curve resulting from the stimulation of *B* alone, also with an opening induction shock, derived from a single Daniell with the secondary coil of a second Du Bois Reymond's machine at 30. The mark x indicates the moment at which the primary currents were broken, being exactly the same in both *A* and *B*. In both cases the direction of the induced current was descending. It will be observed that, allowance being made for the difference in the base lines, the curves are very nearly of equal height. The curve 3 represents the muscle-curve resulting from the simultaneous stimulation of *A* and *B*, x as before marking the breaking of both primary currents. Allowance being made for differences in the base line, the curve 3 is the almost exact duplicate of the curve 2. In other words, when the breaking of both primary currents is simultaneous, the contraction produced by the stimulation of *B* is in no way affected by the stimulation of *A*.

Curve 4 represents the muscle-curve resulting from the stimulation of *A* and *B*, when the primary circuit of *A* is broken as before at x, and the primary circuit of *B* broken at y, the contact breaker of *B* having been shifted 5 mm. in advance of its previous position. The tuning-fork-curve below the muscle-curves marks 180 double vibrations per second, the distance between x and y measured by this gives $\frac{7}{360}$ or $\frac{1}{50}$ second, as the interval of time elapsing between the stimulation of *B* and that of *A*. The muscle-curve is, as will be observed, immensely increased, and at the same time altered in form, so that its maximum point is deferred, being very nearly identical in time with those of the previous curves.

Fig. 2 represents a series of curves obtained by increasing the interval between A and B, B being in all cases thrown in advance of A. Thus the curve 1 is the muscle-curve resulting from the simultaneous stimulation of A and B with the secondary coils in each case at 30 and a single Daniell's to each, x as before marking the moment of breaking the primary currents. Curve 2 is the curve resulting when B is thrown 2·5 mm. ($=\frac{1}{120}$ sec.) in advance of A; the contraction is most distinctly greater than in 1. In curve 3 the interval between A and B is 5 mm. or $\frac{1}{60}$ second; there is still an increase of the curve. In curve 4 the interval is 10 mm. or $\frac{1}{30}$ second, the contraction produced by B has arrived at its maximum before the contraction produced by A has begun, a division into two curves is already apparent, and the total contraction has reached its maximum. In 5 (15 mm. $=\frac{1}{20}$ second) the analysis of the curve has proceeded further, while the total contraction has diminished; and in curve 7 (25 mm. $=\frac{1}{12}$ second) we have simply two curves, the earlier one being that resulting from the contraction of B, the later one being due to A, and all but coinciding with the original curve of A and B.

In all the above cases B was made earlier than A, but exactly similar results are obtained when A is made earlier than B.

With simultaneous stimulation at two different points of the nerve the contraction resulting is the same as when the near point only is stimulated; but when a small interval of time is allowed to elapse between the two stimulations, the contraction increases, rising to a maximum as the interval is lengthened, and afterwards dividing gradually into two independent curves.

What are the inferences which may be drawn from these results? At first sight they might seem to be mere variations of Helmholtz's well-known researches on the summation of contractions. But there is a most important difference. Helmholtz has shewn that when a maximum induction shock (i. e. an induction shock giving a maximum contraction) is made to follow (falling upon the nerve at the same point) a preceding similar shock, no increase of contraction is observed when the interval between the two shocks is so small that by the time

the muscle has begun to contract in obedience to the second shock the contraction due to the first shock has not appreciably begun. This interval he puts at about $\frac{1}{500}$ sec. But this is true of maximum shocks only. Whether applied simultaneously, or with an interval of time between them of any sufficiently small magnitude, two minimum (or at least not maximum) shocks produce a contraction greater than that due to either of the shocks acting singly. In the above experiments the facts illustrated by Fig. 2 have to do with the summation of contractions in the muscle; and would come out exactly the same if a maximum stimulus were employed, the interval between curves 1 and 2 being greater than is necessary to obtain summation of the contractions. The same may also be said of the curve 4 in Fig. 1; the interval there also being considerable. But in such a case as that illustrated by curve 3, Fig. 1, we have to deal with something different. Here the stimulus of both A and B being below the maximum, we ought to have an increase of the contraction on double stimulation; whereas the curve is as nearly as possible of the same form.

Similar considerations shew that the absence of increase of contraction in simultaneous contraction cannot be due to any action taking place at the electrodes of B. For example, we might suppose that the stimulation of B diminishes for a definite small fraction of time the conductivity or irritability of the nerve between the electrodes B in proportion as it excites it to action, that consequently from the nervous impulse originating in A there is taken away when it reaches B just so much as is required to give the contraction due to the excitation of B, and that therefore A and B being equal, the resulting contraction is the same as when B only is stimulated.

This is in itself exceedingly unlikely when we remember that a maximum stimulus is not in question. Granted that B is exhausted (if we may use the phrase) in proportion to the stimulus, there remains, when the stimulus used is below the maximum, a surplus of irritability at B, of which the impulse at A can avail itself, and so produce an increase of the contraction just in the same way as increasing the stimulus at B itself would increase the contraction. Besides, this loss of conductivity or excitability, or this exhaustion, whatever it be called, is coincident with

the stimulation, or at least can last only an exceedingly short time afterwards, for the stimulus repeated at *B* after as short an interval as possible gives rise to an impulse which by summation on the muscle increases the contraction. Hence if the results arrived at were in any way due to such a cause acting at *B*, they ought to be influenced by the distance between the two pairs of electrodes *A* and *B*. If when *A* and *B* are near together no obvious increase of contraction makes its appearance on simultaneous stimulation, yet it ought to become more and more distinct the farther apart *A* and *B* are placed. This however is not the case; the absence of increase of contraction in simultaneous double stimulation holds good at whatever distance apart *A* and *B* are placed.

The only explanation which I can offer is that we have here to deal with a block of nervous impulses. I avoid the word interference, as having already been appropriated by the physicists in a more limited sense. When *A* or *B* is stimulated, a nervous impulse is originated and travels in two directions: downwards towards the muscle and upwards towards the central end of the nerve. The evidences of this double direction of impulses are, 1st, the fact that the negative variation does travel equally in both directions; 2nd, the experiments of Phillipeau, Vulpian, and others, on the mixing of sensory and motor nerves.

When *A* and *B* are stimulated simultaneously the upward impulse from *B* meets the downward impulse from *A*, and the two block each other; i.e. the two impulses moving in opposite directions mutually antagonize each other, so that neither is carried on in its own direction beyond the point where they meet. Consequently, the impulse from *A* not reaching the muscle at all, the contraction in the muscle is caused by the impulse from *B* only. And naturally the effect is the same whatever be the distance between *A* and *B*.

When however one of the two, for example, *B* is stimulated a certain fraction of a second earlier than *A*, the upward impulse of *B* has passed beyond the electrodes *A* on its way to the central end of the nerve before *A* is itself stimulated. Hence the downward impulse from *A* meeting with no opposition reaches the muscle where the contraction it gives rise to is summed up

with the contraction due to the stimulation of *B*, and an increase of the curve is the result.

In the same way, when *A* is stimulated before *B*, the downward impulse from *A* passes *B* before *B* is stimulated, and hence meets with no opposition.

In both cases it is obvious that the result will be affected by the distance between *A* and *B*. When *A* and *B* are near each other the time necessary for the upward impulse starting from *B* (when *B* is stimulated first) to have passed by *A* is less than when *A* and *B* are far apart. The difference however, with the extremest range possible on the sciatic nerve of a frog, is a very small fraction of a second; and the contact breaker which I used was not sufficiently delicate to appreciate this fraction in a satisfactorily accurate manner. I am obliged therefore to postpone the completion of these observations till a new contact-breaker has been constructed for me. I may add that this method evidently offers an opportunity of indirectly measuring the velocity of a nervous impulse.

Until these further observations have been carried out, I cannot claim to have established satisfactorily the theory of a block. Meanwhile I may notice an objection which readily presents itself. If the sciatic nerve of a frog consisted of a single fibre, a block might be intelligible enough. But the nerve is composed of a multitude of fibres, each of which we have reason to think may act independently of the others; and one would imagine that while the upward impulses from *B* were passing along certain fibres, the downward impulses from *A* might pass independently along other fibres, without the one in any way interfering with the other. And one might further suppose, that in any position of a nerve on the electrodes some fibres are excited by a shock passing through the electrodes and others not. But this appears not to be the case. As far as we know, when a nerve is placed between a pair of electrodes 3 mm. apart, all the fibres in that part of the nerve are excited by a stimulus passing through the electrodes. This is shewn by the contraction of all the muscles supplied by that nerve, and not of any particular set or sets of muscles.

I have used the phrase "block" rather than interference, because the latter has a special reference to wave-movements.

The phenomena which I have described are rather those of the mutual neutralisation of two opposing forces. They may be interpreted on either a 'molecular mechanical' theory, or a 'chemical' theory of nervous action. One may imagine, by way of an exceedingly rough illustration, two movements starting from each pair of electrodes and clashing midway, or indeed the block may be felt at the very beginning of the movement and no actual movement affected at all, as when the two extreme members of a series of balls are struck at the same moment. One may equally imagine two chemical transformations stopped at midpoint by a common want of material for the time being, each having exhausted half that of the other, just as the explosion of a train of gunpowder stops in the middle when lighted at both ends. I simply use the phrase, without committing myself to any special interpretation of it.

All the above observations refer to what I have called Case I., where the momentary induced current had a descending direction in both *A* and *B*.

I have also made observations on the other cases. Before making use of the pendulum myographion, I was inclined to believe that the direction of the currents (whether opposing or otherwise) affected the magnitude of the contraction. But careful observations with the pendulum have convinced me that such is not the case, that the direction of the current makes no difference whatever to the general result, that with simultaneous double stimulation there is no increase of contraction. This is shewn by the figures 3, 4, 5, which illustrate Case II., III., IV. respectively.

I have also made observations using the make and break of a continuous current in place of a single induction shock, and likewise on tetanus, but they are as yet incomplete, and I must defer their publication for the present.

NOTE ON THE PRESENCE OF AN INSOLUBLE SUGAR-FORMING SUBSTANCE IN PENICILLIUM.

By A. G. DEW-SMITH, B.A., *Trinity College, Cambridge.*

PENICILLIUM, grown in a solution of inorganic salts with ammonium tartrate and cane sugar, very rapidly manufactures

considerable quantities of both protoplasm and cellulose, but no starch. It seemed to be a matter of interest to ascertain whether any body at all analogous to starch or glycogen could be detected in the mould when grown in large quantities. For, on the one hand, the association of glycogen with living functional protoplasm is at least common, and may possibly be universal (the quantity of glycogen associated with any given quantity of protoplasm varying within wide limits). On the other hand, we have no direct evidence that penicillium growing in saccharine solutions obtains its cellulose by the immediate transformation of the sugar; on the contrary, analogy would rather point to the conclusion that the cellulose is a product of the changes going on in the protoplasm which itself feeds on the sugar. If such a view be correct, one would naturally expect to find in the plant small quantities of carbohydrates analogous to starch, either as stages in the transformation into cellulose or as by-products.

With these views I grew large quantities of penicillium, making use of Mayer's (*Untersuch. über d. alkohol. Gāhrung*) normal solution, Potassic Phosphate ·1 grm., Calcic Phosphate ·01 grm., Magnesic Sulphate ·1 grm., Ammonium Tartrate 15 grm., Sugar, 20 cc of a 15% aqueous solution, Distilled Water 1000 cc.

The spores were sown in this fluid in shallow saucers. In a few days a thick crust of the mould appeared. This was removed before the gonidia had formed; in this way the mycelium was roughly separated from the fluid in which it was growing.

The fluid after filtration and evaporation to a small bulk was treated with alcohol as long as any precipitate occurred. The precipitate was then washed with alcohol until free from sugar. The watery decoction of the precipitate thus freed from sugar gave no evidence of the presence of glycogen, or of sugar after being acted upon by ferment. The ferment used was pancreatic ferment purified by means of glycerine after Von Wittich's method.

The mycelium was thoroughly washed with alcohol until free from sugar. A watery decoction of this washed mycelium was filtered until a clear light yellow solution was obtained.

This decoction gave no evidence of glycogen. Evaporated down to a small bulk, it was treated with alcohol as long as any precipitate occurred. The precipitate washed with spirit and redissolved in water, was also free from glycogen and from any body giving rise to the reactions for sugar after the action of ferment.

The portions of mycelium left after aqueous decoction, and therefore insoluble in water, were treated with alcoholic potash, and afterwards thoroughly washed free from alkali. A quantity of this purified mycelium was boiled in water, and the aqueous filtrate again examined for glycogen or for sugar-forming substances. But none were found.

Up to this point my results were entirely negative. A quantity however of this purified mycelium which was insoluble in boiling water, gave none of the reactions for starch and was not coloured by iodine, was suspended simply in water, a small quantity of ferment perfectly free from sugar added, and the whole left at the temperature of 40°. In a short time the fluid contained considerable quantities of sugar, reducing the cupric solution with great readiness.

There is present then in the mycelium of penicillium a substance which resembles glycogen in not being acted upon by alcoholic potash, and in becoming converted into sugar under the influence of amylolytic ferments, but which differs from glycogen in being insoluble in water, and gives neither the starch nor the glycogen reaction with iodine. In its insolubility it resembles cellulose, but differs from that body in its behaviour towards ferment. An insoluble glycogen has been described as being present in the liver in company with the ordinary glycogen, but it is more than probable the insolubility in this case was due to the fact that the other solid substances protected the soluble glycogen from solution. In the case of the penicillium mycelium such an explanation is invalid. The mycelium was reduced to a fine powder and so thoroughly boiled that everything soluble in water must have passed into solution.

I attempted to isolate this substance in various ways, but entirely failed.

CONTRIBUTIONS TO THE ANATOMY OF THE INDIAN ELEPHANT (ELEPHAS INDICUS). PART III. THE HEAD[1]. By M. WATSON, M.D., *Demonstrator of Anatomy in the University of Edinburgh.*

I HAVE no intention in this communication of writing a full description of the anatomy of the head of the Indian Elephant, but merely to direct attention to certain structures which have either been overlooked by previous anatomists, or have been described as possessing arrangements differing from those which I have met with in the course of my dissection.

TEMPORAL VENOUS RETE MIRABILE.

In *Froriep's Notizen*, October, 1832, p. 39, the following passage occurs:—Otto "showed a drawing of a peculiar *arterial* network, which he found in the neighbourhood of the peculiar excretory gland of the head of the elephant, situated between the eye and the ear, and seemingly occupying the entire side of the head, and remarked that similar arterial retia and anastomoses were to be found in several tardigrades, as well as in the extremities of many plantigrades." The same author, in his *Erläuterungs-Tafeln* (Heft 6), gives a figure of this rete, but in the explanation appended to the plate does not state whether the rete is venous or arterial. As, however, the figure is coloured *red*, we must conclude that he believed it to be an *arterial* network, as in all the other figures blue is the colour used to distinguish the veins when present. Farther, Mayer in his paper (*Nova Acta* XXII.) refers to this description; and although he had himself dissected the animal, does not disagree with it. In my own dissection I found, by means of injection, that this rete was not arterial but *venous*. It is formed by numerous anastomosing veins, which occupy the whole of the temporal fossa, lying superficial to the temporal muscle, but on

[1] Part I. On the Thoracic Viscera, appeared in this *Journal*, November, 1871; Part II. On the Urinary and Generative Organs, in November, 1872. This part was read as a communication at the Bradford meeting of the British Association, Sept. 22, 1873.

a plane deeper than that of the temporal arteries, or that of the peculiar temporal gland. These veins are small in size round the margins of the temporal fossa, and are formed by the junction of numerous rootlets derived from the skin and superficial structures. Increasing in size as they are traced down to the temporal fossa, and communicating freely with one another to form the rete, they finally converge toward the root of the zygoma to form three or four main trunks, which uniting together give rise to the temporal vein. By means of several branches a communication is opened up with the facial vein in front, but this latter trunk takes no part in the formation of the rete, which moreover receives several branches from the substance of the temporal gland. It is worthy of remark that there are no valves present in the veins forming this rete, as the entire network can be freely injected from the trunk of the temporal vein. This trunk finally unites with that of the internal maxillary vein, and both open into that of the *in*ternal jugular, the *ex*ternal jugular vein being absent in the elephant.

The temporal *artery*, after crossing the zygoma, divides into two main trunks, from both of which numerous branches are given off to the surrounding parts, but they have no tendency to form a rete, as described by the authors before referred to. What may be the function of a venous rete in this situation it is difficult to determine.

EYE.

In the next place I wish to draw attention to some interesting points relating to the arrangements of parts within the orbit of the elephant. First, as regards the muscles of the orbit, Mayer (*Nova Acta*) states that there is a depressor of the lower eyelid in this animal, in addition to the other muscles usually contained within the orbit. So far as I can ascertain, he is the only author who has, up to this time, observed the muscle in question. It arises along with the recti and obliqui from the bony canal posterior to the orbit, passing forward beneath the globe of the eye in the same manner as the elevator of the upper lid passes forward above the eye, and is inserted into the cartilage of the lower eyelid. It evidently

depresses the lower eyelid. In addition to this muscle there is farther to be observed a very extensive and well developed periosteal muscle, which has not hitherto been observed in this animal. It corresponds exactly in position to the similar muscle in the sheep and deer[1]. The orbit itself is completed posteriorly and inferiorly by periosteum, and it is in relation with the orbital surface of this periosteum that the muscle referred to is situated. The fibres composing it, which are of the involuntary or non-striated description, form a large sheet covering nearly the whole of the periosteum, and run from without obliquely forward and inward. The function of this muscle, although generally stated to be that of a protractor of the eyeball, is, I think, difficult to determine with precision. Camper, Harrison, and Mayer, refer to two small but distinct muscles, which pass to be inserted into the cartilage of the third eyelid. Of these, one arises from the lower, and the other from the upper eyelid, and both pass inwards to be inserted into the cartilage just referred to. On careful dissection I have not found them to be distinct muscles, but formed by certain of the palpebral fibres of the orbicularis palpebrarum, which pass inward to be inserted into the third eyelid. According to Mayer, one of these muscles acts by drawing the third eyelid outwards across the globe of the eye, whilst the other retracts it toward the inner canthus. That this is the action of these muscles seems to me extremely doubtful, as both being formed of prolongations of the orbicularis, it is difficult to imagine that two parts of the same muscle supplied by one nerve (the 7th) should have actions so opposed to one another. So far as a study of the anatomical arrangement of the parts would enable me to decide, I am inclined to think that *both* those muscles will tend by their contraction to draw the third eyelid outwards across the eye, but by what agency this lid again regains its position is more difficult to determine. It would be interesting to learn from those who may have opportunities of watching the elephant during life, whether the third eyelid is drawn across the eye when the upper and lower eyelids are separated, or

[1] See Professor Turner's papers on the Periosteal Muscle of the Orbit in Man, the Sheep and Deer, in *Proc. Roy. Physical Soc. of Edinburgh*, Dec. 19, 1861, and *Natural History Review*, Jan. 1862.

only when these are closed. If the latter supposition be correct, it will establish the views I have advanced regarding the action of these little muscles.

Regarding now the lachrymal apparatus of the elephant, we find that various statements have been made by different authors. Camper and Harrison, on the one side, maintain that no portion of a lachrymal apparatus is present in the elephant, while, on the other hand, Mayer (the most recent writer on the subject) says, "The puncta lachrymalia are small, the lachrymal duct single and very narrow, the lachrymal gland of tolerable size. Its excretory duct is as large as a knitting needle, and opens on the external angle of the eyelids;" and he adds, "It is striking that Camper should neither have discovered this gland, its excretory duct, nor the lachrymal canal." Perrault also mentions the presence of lachrymal glands in the elephant. My own observations agree with those of the authors who have not discovered any portion of a lachrymal apparatus, although each separate element was carefully looked for. It is difficult to explain the statements of those authors who maintain the existence of such an apparatus, more especially when it is borne in mind that the ethmoid bone in the elephant is quite imperforate, and consequently affords no way of escape for the lachrymal secretion. True, a Harderian gland, similar to that which exists in connection with the third eyelid in birds, is to be found in the elephant. It does not, however, occupy the usual position of the lachrymal gland at the outer angle of the orbit, but rests between the inner wall of that cavity and the internal rectus muscle. Its excretory duct, moreover, opens on the surface of the third eyelid, and not in the usual position of the ducts of the lachrymal gland. That this gland to some extent fulfils the function of the lachrymal gland is rendered probable by the statements of African travellers, one of whom (Cumming) describes an elephant, after suffering from the effect of several balls, as weeping profusely. The mode, however, in which the secretion of this gland is got rid of, under ordinary circumstances, is difficult to determine in the absence of all trace of an excretory apparatus.

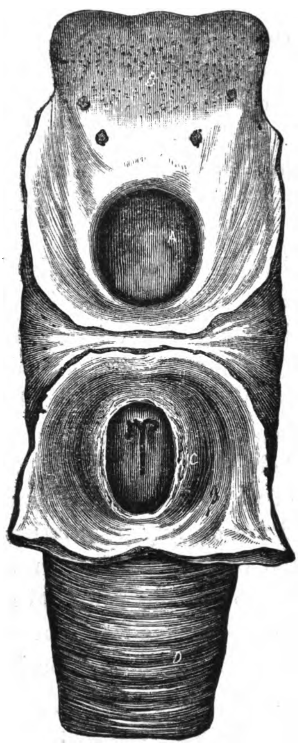
Throat.

I shall now direct attention briefly to certain peculiarities in the formation of the throat of the elephant, which up to this time have escaped notice, and which seem to be of importance, inasmuch as they afford an explanation of some of the creature's functions which have not hitherto been explained. In my first communication I had occasion, when remarking on certain peculiarities in the thoracic viscera of the elephant, to refer to the following statement made by Sir Emerson Tennent in his work on Ceylon, to prove that the animal possesses the power of withdrawing water stored within the cavities of the stomach by means of the trunk inserted into the mouth. Sir E. Tennent says, with reference to the Indian elephant, "I have elsewhere described the occurrence to which I was myself a witness of elephants inserting their probosces into their mouths and withdrawing gallons of water, which could only have been contained in the receptacle figured by Camper and Home;" and he (Tennent) farther quotes from the author of the *Ayeen Akberry* as follows: "An elephant frequently with his trunk takes water from his stomach and sprinkles himself with it, and it is not in the least offensive."

That the same thing is true as regards the African elephant has been observed by Cumming, who, in his travels in South Africa, when speaking of these animals, says: "They seemed heated by the pace at which they had retreated, and were now refreshing themselves with large volumes of water which Nature enables them to discharge from their capacious stomachs and shower back upon their bodies with their extraordinary trunks." This regurgitation of water from the stomach I showed in my first paper to depend not on any peculiarity of structure in the elephant as compared with that of other animals, but that it was a function similar to the physiological regurgitation of food in the ruminant, and performed by means of the combined actions of the diaphragm and other abdominal muscles. It now remains to show in what the peculiarity of construction of the throat of the elephant consists to enable the trunk when placed in the mouth to withdraw the water regurgitated from the stomach. For it is evident that were the throat of this animal

similar to that of other mammals, this could not be accomplished, as the insertion of a body, such as the trunk, so far into the pharynx as to enable the constrictor muscles of that organ to grasp it, would at once give rise to a paroxysm of coughing, or were the trunk merely inserted into the mouth, it would be requisite that this cavity be kept constantly filled with water at the same time that the lips closely encircled the inserted trunk. The formation of the mouth of the elephant, however, is such as to prevent the trunk ever being grasped by the lips so as effectually to stop the entrance of air into the cavity, and thus at once, if I may so express it, the pump-action of the trunk is completely paralysed. We find therefore that it is to some

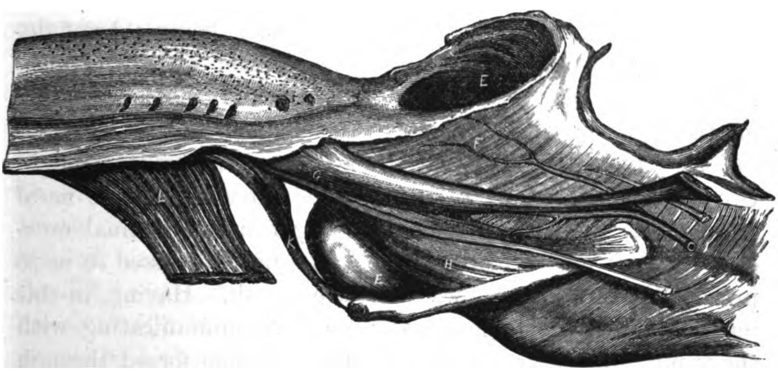
Fig. 1.



Explanation to Figure 1. A, superior aperture of pharynx. B, root of tongue. C, soft palate with larynx projecting through the centre. D, pharynx.

modification of the throat that we must look for an explanation of the function in question, and this we find to be as follows:— The superior aperture of the pharynx, Fig. 1 A, is extremely narrow, so much so as to admit, with difficulty, of the passage of the closed fist. Immediately posterior to this narrow aperture the pharynx dilates into a pouch of large size, Fig. 2 E, capable of containing a considerable quantity of fluid. This pouch is prolonged forward beneath the root of the tongue, and is bounded in the following manner. The floor extends from the epiglottis as far forward as the root of the tongue, being formed from behind forward by the thyroid cartilage, thyro-hyoid membrane, and hyoid bone. Its lateral walls are completed by the sides of the pharynx (that is, by the superior constrictor muscles, Fig. 2 F), in addition to the stylo- G, and hyo-glossi H, muscles. The root of the tongue forms the anterior boundary, whilst the posterior wall is completed by depression of the soft palate, or when the latter is elevated the pouch then communicates freely with the œsophagus. In connection with this pouch is to be observed the very peculiar form of the hyoid bone, which being deeply concave on its upper surface forms as it were the greater part of the floor of this pouch. Between the pouch and the concavity of the hyoid bone, moreover, there is placed a large quantity of loose and distensible connective tissue, which permits of the expan-

Fig. 2.



Explanation to Figure 2. E, E pharyngeal pouch. F, superior constrictor. G, stylo-glossus. H, hyo-glossus. K, small muscle which diminishes the depth of the pharyngeal pouch. L, genio-glossus muscle.

sion of the pouch. The size of the latter is, moreover, liable to alteration by the actions of several muscles. These are more especially the hyo-glossi muscles, and two little additional muscles, Fig. 2 K, the homologies of which I have not yet been able to determine, which, springing from the middle line of the hyoid bone in front of the pouch, pass up, one on either side of the middle line, and blend with the other muscles forming the root of the tongue. By the action of these muscles the pouch may be diminished in *depth*; but in consequence of the narrow interval existing between the hyoid cornua, the *length* of the pouch from before backwards cannot be altered, as the thyroid-cartilage is thereby prevented from being approximated to the hyoid bone. I have now to complete the anatomical description of this pharyngeal pouch by a reference to the formation of the soft palate. This, Fig. 1 C, which is of very large size, forms almost a complete muscular diaphragm, through the central aperture of which projects the superior extremity of the larynx, which thus in some respects approximates to the arrangement of the corresponding parts in certain cetacea as described by Dr James Murie[1]. With reference to the muscles entering into its formation, we find that the palato-glossus is entirely absent, its place being supplied by a wide and extremely distensible fold of mucous membrane. The palato-pharyngeus, on the other hand, is of large size, and forms, in fact, the principal feature in the soft palate. There is neither a levator nor a tensor palati present. Such being a brief description of the anatomical arrangements met with in connection with this pharyngeal pouch, a few words may now be said on their physiological bearing. An elephant can, as the quotations sufficiently prove, withdraw water from his stomach in two ways; first, it may be regurgitated directly into the nasal passages by the action of the diaphragm and abdominal muscles, the soft palate being at the same time depressed so as to prevent the entrance of water into the mouth. Having, in this manner, filled the large nasal passages, communicating with the trunk, the water contained in them is then forced through the trunk by means of a powerful expiration; or, in the second place, the water may be withdrawn from the cavity of the

[1] *Trans. Zoolog. Soc. Lond.* Vol. VIII.

mouth by means of the trunk inserted into it. Now, in this case, it is manifestly impossible that the water can be contained within the cavity of the mouth itself, as I have already shown that the lips in the elephant are so formed as effectually to prevent this. The water regurgitated is, however, by means of the elevation of the soft palate, forced into the pharyngeal pouch. The superior aperture of this pouch being much narrower than the diameter of the pouch itself, and being completely surrounded by the muscular fibres of the stylo-glossus on each side and the root of the tongue in front, which is prolonged backwards so as to form a free sharp margin, we have thus as it were a narrow aperture surrounded by a sphincter muscle, into which the trunk being inserted and grasped above its dilated extremity by the sphincter arrangement just referred to, air is thus effectually excluded, and the nasal passages being then exhausted by the act of inspiration, water is lodged within these passages to be used as the animal thinks fit, either by throwing over his body or again returning it into his mouth, as observed on one occasion by Cumming, who says: "Throughout the chase this elephant repeatedly cooled his person with large quantities of water, which he ejected from his trunk, over his back and sides; and just as the pangs of death came over him, he kept pouring water into his bloody mouth until he died." The increase in the size of this pouch is accomplished mainly by the depressor muscles of the hyoid bone, which, in consequence of the tongue being fixed and restrained in its movements by the muscles attaching it to the lower jaw, depress the hyoid bone and thyroid cartilage, which are both freely moveable, at the same time that the tongue itself is almost fixed, and in this manner the depth of the pouch is materially increased. Such is the explanation of a function which, so far as I am aware, has not up to the present time been satisfactorily explained, and it will be of much interest to examine the corresponding region in the African elephant, to ascertain if arrangements similar to those I have described in the Indian species are to be found in that animal. If similarity of function implies similarity of structure, then I have little doubt that such will prove to be the case. The modifications in the throat of the elephant are not without interest from two points of view.

In the first place, these modifications are such as show that, in respect of this portion of its structure as in several others, the elephant closely resembles certain forms of the cetacea, and are thus of importance inasmuch as they furnish one more item of evidence in favour of a relationship which has been long suspected to exist between those two groups, which when taken by themselves, although they seem sufficiently widely separated, are, nevertheless, connected by a number of intermediate forms: and, secondly, they are not without interest inasmuch as they afford food for reflection as to the origin of these modifications when compared with the corresponding parts of other mammals. Did they arise gradually in accordance with the law which tends ever to bring the organism into harmony with external conditions, and so to adapt the functions of such an organism as finally to give rise to that chain of circumstances which is formulated in the expression, "survival of the fittest"? Or are we to believe, with the teleologists of the previous century, that these modifications were occasioned by the direct intervention of a great First Cause ever attempting to remedy imperfections which he had at first created? Which of those alternatives is to be accepted must be left to the private judgment of each individual enquirer.

ON THE APPARENT PRODUCTION OF A NEW EFFECT BY THE JOINT ACTION OF DRUGS WITHIN THE ANIMAL ORGANISM. By T. LAUDER BRUNTON, M.D., D. SC., Edin., *Lecturer on Materia Medica and Therapeutics at St Bartholomew's Hospital.*

THE admirable researches of CrumBrown and Fraser have demonstrated that the physiological action of several alkaloids may be completely altered by their union with such bodies as iodide of methyl. The compounds thus produced sometimes act on organs which do not appear to be affected by either of the components separately, the ends of the motor nerves for example being paralyzed by iodide-of-methyl-strychnia, though neither iodide of methyl alone, nor strychnia alone, seems to have much influence over them. Although chemical action outside the body alters in this way the action of alkaloids, I am not aware that any instance has been noticed in which a similar modification appears to be produced by the joint action of two drugs after their introduction into the animal organism. I have lately observed an example of this sort in the case of strychnia and nitrite of amyl. The experiments which I made on this subject were performed in several ways, but I will only describe the two most important. In the first series of experiments a solution of strychnia was injected into the dorsal lymph sac of a frog, and as soon as tetanus came on the animal was put into a vessel filled with the vapour of nitrite of amyl. A second healthy frog was also introduced along with it for the purpose of comparison. They were left in the vessel till both were motionless, when they were removed and the sciatic nerves of both were exposed. On irritating these nerves by the application of a Faradic current, vigorous contractions occurred in the limbs of the frog poisoned by nitrite of amyl alone, but those of the animal poisoned by strychnia and nitrite of amyl together remained motionless. The skin was then removed from the legs of both and the muscles irritated by the application of the current

directly to them. In many instances, those of the frog poisoned by the nitrite and the strychnia together contracted nearly as strongly and readily as those poisoned by the nitrite alone. It is therefore evident that their failure to contract when the nerves were stimulated must have been due to paralysis of the nerves themselves, just as it is in poisoning by woorara. The second series of experiments was made by ligaturing the artery supplying one leg of a frog before injecting strychnia into the lymph sac. The poison was thus carried by the blood to every part of the body except the leg whose artery had been tied. The animal was then placed in a vessel filled with the vapour of nitrite of amyl as before, and after motion had ceased the sciatic nerves were exposed and irritated. It was then found that the muscles of the ligatured leg which had been exposed to the nitrite of amyl, but preserved from the strychnia, contracted vigorously when the corresponding sciatic was irritated, while those of the other leg did not respond at all. When the skin was stripped off, however, and the muscles irritated directly, in many instances no great difference could be noted between their irritability.

The muscles of frogs which had been poisoned either with strychnia and nitrite of amyl, or with nitrite of amyl alone, passed more quickly than usual into a state of rigor mortis; and I therefore regard nitrite of amyl as a muscular poison. It is not improbable that the apparent paralysis of the motor nerves may be partly due to diminution of the irritability of the muscle itself, but the results of direct stimulation show that this is not sufficient to explain it entirely, and we must therefore believe that the nerves themselves are also paralyzed. Besides nitrite of amyl, I have tried the nitrites of sodium, ethyl, butyl and capryl; but my researches on these are not yet completed. They seem however to show that the nitrites are muscular poisons, but their actions differ according to the bases which they contain.

AN ACCOUNT, HISTORICAL AND PHYSIOLOGICAL, OF THE MADAGASCAR ORDEAL POISON, THE *TANGHINIA VENENIFERA*. By ANDREW DAVIDSON, F.R.C.P.E., *Medical Missionary and Physician to the Court of Madagascar.*

ORDEALS of various kinds have been devised in certain stages of civilization as a means of testing the guilt or innocence of suspected persons. Records have come down to us of the widespread existence of this usage in remote antiquity, and in more recent times ordeals by fire, water, and wager of battle were prescribed by law and sanctioned by religion throughout the whole of Europe. Ordeal by *poison* is, however, peculiar to Africa, although philology renders it probable that the same practice may have prevailed among the progenitors of our own race in prehistoric times.

It is to be observed that these ordeals are chiefly employed for the detection of witchcraft, by which African Jurists understand the use of poisonous drugs for evil purposes. It is in fact equivalent to the *φαρμακεία* of the Greeks; and as the terms *φαρμακὸς* and *veneficus* were applied by the ancients to signify alike a physician, a sorcerer, and a poisoner, so in many of the African languages the same peculiarity obtains. This arises from the fact that among these and other primitive races the physiological effects of drugs, whether poisonous or medicinal, are ascribed to some magical power, either inherent in the substance itself, or imparted to it by sorcery. Medicines are thus employed as charms both for causing and curing disease. With such superstitious notions of the properties of poisons, it was only natural that they should ascribe the differences in the results observed to follow their administration to a sort of discriminative faculty or intelligence possessed by the substance, and thus have come to employ poisons in the detection of occult crimes, such as witchcraft.

Although we know that the custom of ordeal by poison prevails over a great part of the continent of Africa, we are

as yet unfortunately ignorant, in most instances, of the poisons employed by the different tribes[1]; and, with the exception of the Calabar bean, none of them have been subjected to a satisfactory examination. This is to be regretted from a medical as well as a scientific point of view, as remedial agents of high value will probably be found among these powerful ordeal poisons.

As the advance of civilization has now abolished the use of one of the most celebrated of these ordeals—the '*Tangèna*' of Madagascar—it seems desirable to put on record the mode of administering it and its effects on man, while such information may still be obtained from those who were acquainted with its employment, and had witnessed or experienced its effects. Shortly before my arrival in the island, in 1862, the Tangèna ordeal was abolished, but as it happens that an officer, now attached to the hospital, was formerly from hereditary office an administrator of the poison, I have in this way had the opportunity of obtaining trustworthy information upon this subject.

Historical.—There is no certain evidence when or how the Tangèna first came to be used as an ordeal in Madagascar. We know that some such method of trial has long been practised in the Island. The testimony of Flacourt, who visited Madagascar in the middle of the 17th century, is conclusive upon this point; but if his statements are to be considered as strictly accurate, some other poison must at that time have been used in the district visited by him[2]. Ordeals of other kinds, such as that by plunging the hand into boiling water, were at one time practised in some parts of the country; and there seems reason to believe that the Tangèna was not generally or frequently employed until the beginning of the present century.

[1] Dr Livingstone observes that this custom "is common among all the Negro nations north of the Zambesi." The natives of that part of Africa employ a plant called the 'Goho' which is possessed of purgative and emetic properties.

The poisonous juice of the Erythrophlaeum Guineense is employed for the same purpose on the coast of Guinea, and the Physostigma venenosum by the natives of Calabar. In the inland regions near the equator, according to Du Chaillu, the natives use as an ordeal the root of a plant or tree called "Mboundou" conjectured by Prof. Torry of New York to be a species of strychnos.

[2] He says that the Malagasy administer for this purpose "Manrechetsi, qui est de quelque sorte d'herbe ou de racine qui est poison et fait mourir celui qui en mange." *Histoire de la grande île de Madagascar.*

It was seldom had recourse to in ordinary judicial cases in which more rational modes of trial were followed, but was reserved for the detection of those guilty of infamous crimes, for the discovery of whom ordinary evidence either could not be obtained or would not suffice. Such crimes were treason and witchcraft, and indeed the latter comprehended the former, and for the detection of these it was administered either by order or permission of the sovereign, and in presence of officers appointed by him. An experiment was, however, frequently enough made, *in corpore vile*, in the instance of individuals suspected of minor offences, or in order to decide which of two or more persons was guilty of a crime known or believed to have been committed by one or other of them.

In the former case, a dog having been selected as a substitute for the suspected party, the Tangèna was given to the animal in the same way as when administered to a human being. When again it was given with a view to decide between two or more accused persons, then dogs of similar size and condition were selected, and the party whose representative first succumbed to the poison was treated as guilty.

Mode of administration.—The ordinary mode of administration was as follows:—Two Tangèna almonds, or nuts as they are often styled, were taken, and the half of each rubbed down with water. This custom of taking the half of two different almonds was adopted in order to increase the probabilities that the poison administered should be only of the average strength.

The suspected party now ate a little rice and was afterwards made to swallow three small pieces of fowl's skin, and this was followed by the Tangèna emulsion. After a few minutes, varying however according to the result desired by the administrator, tepid water was given in considerable quantities, and violent, long-continued vomiting usually ensued. If the three pieces of skin were discharged the suspicion of guilt was dismissed, as a rule[1], and the friends of the unfortunate were then left to do their best for his recovery. Not unfrequently, how-

[1] I say "as a rule" for some other omens of an unfavourable kind, which I do not require to detail here, occasionally affected the result.

ever, the poison operated more as a purgative than as an emetic, and then it often happened that with or without the stigma of crime (according as the pieces of skin were retained or rejected) the case terminated fatally. It can easily be understood that state policy readily attained its crooked ends by the administration of the Tangèna. It was observed that those who might be called "*the opposition members of the government*" seldom recovered from the ordeal. So far as can be ascertained, it proved fatal in as many as one in ten cases when given with no hostile intention. As it was often administered to whole villages at once, it will be understood that the numbers destroyed by this poison were immense.

The points especially affecting the result seem to have been:—(*a*) The colour of the kernel; the very red ones are said, and probably with truth, to be more poisonous than the less ripe ones, which are whiter in colour. (*b*) The amount administered was in every case enough to prove fatal, if not speedily rejected. From what I have learned from the natives, as well as from the results of my own experiments, I have no doubt that the weight of one almond is amply sufficient to poison an adult, if not got rid of by vomiting. (*c*) If administered on an almost empty stomach, it was more dangerous than when a larger quantity of rice had been previously taken. (*d*) A great deal depended upon the seasonable and reasonable administration of diluents. Experience enabled the expert to judge the time when to give drink, and the amount required to effect his object, whether that might be the death or recovery of the victim.

Symptoms.—As the result of a careful examination of several who have been themselves subjected to this ordeal, and of many who have witnessed its effects on others, I conclude that the symptoms produced by it when given in poisonous doses, in the manner just described, are as follows:—A peculiar numb tingling sensation is felt in the mouth and fauces, due to its topical action. Several of those who have undergone the ordeal have assured me that they experienced a similar feeling more or less over the whole body, but especially in the hands. This point is important, for my experiments on warm-blooded

animals have not indicated any noticeable disturbance of sensation. Sickness ensues with vomiting, intense, distressing and repeated—first of the contents of the stomach—then of bile and mucus. The vomiting is attended by a feeling of great debility and anxiety. If the greater part of the poison has been thus ejected, the patient recovers perfectly within a short time. Where more of the poison has got into the circulation the sufferer is said to feel giddy. The Malagasy, however, use their word for vertigo in a loose sense. I am therefore inclined to think that partial paralysis of motion with unsteady gait may be the condition indicated. The patient, under the influence of the Tangèna, staggers if he attempts to walk, is unable to support his own weight, and falls down helpless and paralysed. Although the mind is usually clear, yet delirium *occasionally* occurs. Along with these nervous and cerebral symptoms, purging and urination appear and are more or less urgent. The fæcal discharges do not contain blood or mucus. Nothing abnormal has been observed by the natives in the appearance of the urine. The patient in cases tending to a fatal issue becomes unable to rise. In other instances, according to the testimony of observers, he lies as if asleep, and when roused answers like a drowsy man, then lapses back into his former condition. In other cases the patient remains conscious to the last, without either stupor or delirium. Death is preceded by spasmodic movements of the fingers and toes. Purging is a bad symptom and worse the more urgent it is. Almost none recover when the stage of stupor has been reached.

The natives know of no antidote for this poison, but they think that the application of cold and draughts of lemon-juice are of service.

I may remark upon the condition of sleepiness described above as of pretty frequent occurrence in the advanced stage of poisoning by this substance, that I do not believe that my informants were able to distinguish between narcotism and a state of prostration. I have however given their statements literally.

Upon this point I may further observe that my experiments on the lower animals do not seem to countenance the opinion of some, that there is any narcotic property in the substance,

There is only one exception to this statement of the result of my experiments. In two instances in which I administered the Tangĕna to fowls, they appeared to be overcome by sleep.

No post-mortem examination has been made of those who have died by Tangĕna.

Botanical note.—The Tangĕna, or Tanghin (*Tanghinia Venenifera; Cerbera Tanghin*, Hooker), is a large tree of the natural order *Apocynaceae*. The poisonous part of the plant is the fruit, which is a *drupe*, almost the size of an apple. The colour of the fruit is a greenish-yellow; the external pulp which surrounds the kernel is soft, somewhat grey in colour, destitute of smell, and possessed of a slightly bitter, disagreeable taste. The kernel is hard, ligneous, and brown, and elliptical in shape. Within this is the almond, which is divided into two cotyledons, of the consistence of a newly-plucked bean, varying in colour from a white to a brownish-red; and weighing from forty to seventy grains. For a minute botanical description of the tree and its fruit the reader may consult Hooker's *Botanical Miscellany*, III. 290. The Tangĕna grows abundantly in the forests on the east coast of Madagascar, it is rare in the central provinces, and towards the south of the island.

Chemistry.—Two crystalline principles are said to have been obtained from the Tangĕna: the one, the bitter principle *Tanghinin;* the other, the poisonous principle which has been named *Tanghicine*, and is described as transparent plates obtained by æther, insoluble in water, bitter and poisonous. I have no access however to any account of the process followed in the separation of these; and I shall state in a few words what little I know upon this point.

(a) The kernel contains a large quantity of an inert, bland oil, and if rubbed up with water it forms a white emulsion.

(b) Its active principle is insoluble, or at least nearly so, in water, readily soluble in alcohol, æther, and chloroform, as is proved by the activity of the extracts made by means of these solvents.

I have obtained by means of chloroform impure crystals, in the form of long, flattish needles, arranging themselves under the microscope as if branching out at acute angles from a centre.

(c) By treating a carefully prepared alcoholic extract with water a white precipitate is obtained.

In my experiments, I have used the simple emulsion, and extracts made with æther or alcohol, and in a few instances the impure crystals mentioned above[1].

[1] I have given the above as it was written more than three years ago. As my object was not to investigate its chemical properties, but its physiological action, I had neglected to note down the *steps* of the process by which I obtained the crystals alluded to; and I cannot now add anything from memory. The recent work by Chatin (*Recherches pour servir à l'histoire botanique, chemique et physiologique du Tanguin de Madagascar*. Par Joannes Chatin, Paris, 1873) upon this poison would render it probable that the active principle

Physiological Action.—Results of experiments on the lower Animals.

The Tangèna proves fatal by absorption however introduced, whether into the alimentary canal, the serous membranes, or into the cellular tissue. It acts less actively if swallowed, because partly got rid of by vomiting. When a concentrated solution of the poison is applied to the frog's foot it is slowly absorbed and causes death. Ligature of the blood-vessels prevents or delays its action.

(A). *Topical Effects.*

1. Its topical action on the mucous membranes is to alter and diminish the sensibility in the part. The same results follow its application to the skin.

2. Its local action on muscular tissue is no less evident. Applied to the exposed heart of the frog it produces *immediate paralysis.* It acts with equal rapidity on the excised heart which is still pulsating. The electric contractility of the poisoned muscle is diminished or destroyed. Applied for about thirty minutes to the leg of a frog, the part is paralysed, and if pricked or pinched, is insensible or nearly so; all the while the animal is lively, and the blood continues to circulate naturally in the web of the affected foot. After the lapse of about an hour, the other leg becomes paralysed, then the upper extremities, and finally death occurs in an hour and a half, or two hours.

3. When applied to the exposed sciatic nerve, paralysis followed by death has also been the result; but I am unwilling to deduce any conclusion from this, as, from the difficulty of

is *not* a neutral body, but an alkaloid which he obtained thus:—Having first got rid of a considerable part of the oil by pressure, he made an ætherial extract, which was treated by warm alcohol, and on evaporation left a residue which he thus describes: "la liqueur évaporée dans la vide laissa un résidu assez considérable, brunâtre, légèrement amer et comme granuleux en certains points; facilement fusible, ce produit, chauffé au contact de l'air, se comportait comme un corps gras. Le produit ainsi obtenu était toxique; je le traitai alors par de l'acide acétique étendu et j'obtins, par l'évaporation des liqueurs, une petite quantité de poudre blanchâtre, assez soluble dans l'eau, beaucoup plus soluble dans l'alcool. Elle fut en conséquence traitée par ce dissolvant et, par l'évaporation dans la vide, elle donna de petits crestaux, d'un blanc vitreux et appartenant au système diclinorhombique" (p. 30). This subject will still require further investigation.

localizing the poison, I am not quite satisfied that it did not come into contact with the exposed muscles.

4. When applied to the conjunctiva it does not affect the pupil.

(B). *Action through the Circulation.* (*On warm-blooded Animals.*)

1. However introduced it produces violent vomiting and purging in all animals capable of these actions. As an emetic, Tangḕna is even more violent, and operates sooner when introduced directly into the circulation than when swallowed.

2. It produces marked paralysis of motion, apparently more intense at first in the lower extremities, and last of all affecting the muscles of the trunk and neck[1].

3. When it acts through the circulation, sensation seems slightly if at all *paralysed.* Pinching the tail of a lemur makes it suddenly exhibit signs of pain, long after it is incapable of motion. Other facts seem to prove that sensation is *altered, not abolished.*

4. The animal remains conscious and observant to the last; excepting in fowls, as already mentioned, I have never seen narcotism induced.

5. The action of the heart is first somewhat increased (at least in some cases), then it becomes weak, irregular and slower, and finally stops a short time before the respiration. The heart's action is arrested in systole. The ventricles usually contain a little blood but are never distended. The blood is sometimes slightly coagulated, but more commonly fluid. The auricles and venæ cavæ are engorged.

6. In one case I noticed peristaltic movements of the right auricle, continuing for half an hour, or longer, after death, excited by mechanical or chemical stimuli; and in another case, this peristaltic movement was observed to occur *spontaneously*, twice a minute, producing a wave-like motion in the venæ cavæ.

[1] It must be remarked, however, that in these experiments the poison was injected into the cellular tissue over the loins. This fact should be kept in view as it suggests an explanation of this observation.

7. Idio-muscular twitchings of the pectoral muscles are commonly observed if the body be examined soon after death.

8. After death by Tangèna, I found the muscles to contract on applying strong magneto-electric stimulation, but less perfectly and persistently than natural; but after the direct application of the poison to a muscle, its irritability by electricity is much more distinctly diminished, or even destroyed.

9. The liver is always much congested, and notwithstanding the urgent vomiting, the gall-bladder is sometimes full. There are usually no signs of irritation, or injection of the mucous membrane of the stomach or bowels. There is no congestion of the brain or cord. The lungs are always exsanguine and collapsed.

(C). *Action on Frogs.*

1. However introduced, Tangèna tetanizes the heart, with varying degrees of rapidity, according to the size of the frog, the amount of poison used, and the mode of administration.

The pulsations usually first increase in number, then become less frequent, while from the first they tend to be irregular—afterwards this irregularity becomes more marked. After some time, tetanic contractions of distinct portions of the substance of the ventricle take place, the tetanized portions remaining pale; then complete tetanus of the organ ensues, preventing all further entrance of blood. The auricles continue to contract after the ventricle has ceased. The ventricle sometimes stops for a little and then begins to act again. After death the auricles are distended and the ventricle is contracted and pale. As already stated, the heart's action is immediately arrested by the topical application of the poison to its substance. In this case it is paralyzed not tetanized.

2. The heart ceases to beat before respiration and reflex action are abolished. The decapitated frog, whose heart has been arrested by Tangèna, will draw up its legs on being irritated, unless indeed the paralysis of the extremities has been very complete before the experiment has been tried.

3. Destruction of the medulla, or decapitation, does not prevent, although it perhaps somewhat delays, the action of Tangèna on the heart.

4. The pulsations of the posterior lymphatic hearts become rapid, irregular and excessively weak, and seem to cease at the same time that the heart's action is arrested.

5. The extremities are always more or less paralyzed. After a pretty large dose, the frog remains with its posterior extremities extended, and it requires strong stimulation to excite any movement.

6. Reflex action is probably *diminished, not immediately*, but soon after the administration of the Tangèna. This I ascertained by suspending two frogs, of the same size, by the lower jaw. I gave one of them a small dose of Tangèna (a grain of the semi-liquid spirituous extract), and then by the application of similar mechanical and chemical stimulants, observed the readiness with which in either case reflex movements followed.

7. The muscles can be made to contract by the magneto-electric current after the paralyzed limb has been amputated, but less actively and for a shorter time than in a healthy one. If a ligature be applied to the sciatic artery and vein, the limb below the ligature will be protected from the poison and will contract actively, if the magneto-electric current be applied; while the poisoned limb remains motionless, or nearly so, whether the stimulus be applied through the nerve, or to the muscle.

Experiments on warm-blooded Animals.

Exp. I. *On a Lemur.*—At 7.58 A.M. Ten grains of a liquid extract of *Tanghinia venenifera*, mixed with one drachm of water, were injected into the cellular tissue in the lumbar region of a full-grown lemur. 8 A.M. The extremities are weak—scarcely able to walk. 8.7. Vomits frothy mucus. 8.15. Continues to vomit, and when it attempts to walk, its movements are slow and uncertain, it is unwilling to move; the heart's pulsations are reduced in frequency, pupils normal. 8.17. Lying on its belly with its legs stretched out and flaccid, sensation perfect. 8.24. Limbs paralysed; pinching the tail makes it look round, and it makes vain attempts to change its position, pulsations of the heart less frequent and weaker. It is perfectly conscious, for although unable to move its limbs, it follows with its eyes the movements of any one approaching it. 8.30. Heart irregular, two pulsations and a pause; pupils a little contracted (?), sensation unaffected. The heart is now becoming slower

and slower, the respiration panting. 8.35. The heart only beats occasionally, sensation seems perfect, it is able to move its head a little, but the rest of the body is paralyzed. 8.40. Respiration panting, and forty per minute. 8.45. Paralysis complete, sensation unaffected, the pulsations of the heart very infrequent, after a considerable interval a few hurried beats succeed each other. Slight shivering movements in the tail, fingers and toes (*spasmodic*). 8.47. The heart has ceased to beat, the pupils are dilated, the lower jaw has fallen, a few gasping respirations. 8.47. Slight spasmodic movements in the feet. 8.48. Died.

9.4. Opened the body and observed spontaneous twitchings of the pectoral muscles, which also were excitable by mechanical irritation. Auricles and great veins very much congested; a little semi-coagulated blood in right ventricle; a drachm of fluid blood in left ventricle, which coagulated after removal. For half an hour after death the application of dilute sulphuric acid or mechanical irritation produced contractions, especially of the auricles, which from being dark red became temporarily pale and bloodless; pinching the substance of the ventricles made them contract, the contraction of the latter did not extend to the auricles; lungs collapsed and pale; liver congested, gall-bladder full, vena porta congested, stomach and alimentary canal healthy.

Exp. II. *On a Cat* (nearly full-grown).—After applying a spirituous extract of *Tanghinia v.* to the conjunctiva in order to observe its action on the pupil, with a negative result, and having ascertained the pulsations of the heart to number nearly 120 per minute, at 12.15 P.M. we injected three grains of the same extract, mixed with one drachm of water, into the cellular tissue. 12.20. The animal is able to walk, but its legs seem weak. 12.31. Begins to vomit. 12.37. The action of the heart weak, pulsations about 110 per minute, the vomiting urgent, bowels moved. 12.43. Vomiting still continues, great debility, the pulsations of the heart very much reduced in number and strength, the respirations fewer and panting, pupil normal. 12.50. The animal is now scarcely able to move about, it lays its head down upon the ground, and after a little changes it into a new position, without moving its body. How far this condition results from paralysis, and how far from pure debility, it is difficult to say. 12.52. Pulsations of the heart 20 per minute, and weak. 12.54. Clonic spasms of extremities, with trembling motion of the skin of the back, expulsion of urine and fæces, a sigh, the pupils dilated, the action of the heart has ceased, a few gasps, and the animal died at 12.57.

Examination immediately. Lungs pale and collapsed, the coronary veins of the heart full, the substance of the heart congested, all the chambers contain blood, the auricles however are *engorged*, the left one full of bright red and watery blood, the venæ cavæ full. The liver, vena porta and its branches, are very much congested, the whole intestinal canal abnormally pale. A mild magneto-electric cur-

rent produces contractions of the muscles to which it is applied, the contractions are less powerful when the current is transmitted through the nerve, slight contractions can be produced by connecting the two limbs, the heart does not respond to the magneto-electric stimulus. I have observed paralysis to be much less distinctly marked in the cat than in the lemur.

Exp. III. *On a species of Civet about the size of a small cat.*—The poison mixed with a little water was injected into the cellular tissue at 9.24 A.M. At 9.25, makes violent efforts to vomit. 9.28. Restless, and vomiting. 9.37. Posterior extremities are paralyzed, sensation seems unaffected. 9.40. A slight shivering motion all over its skin; extremities paralyzed, but it is still able to move its head a little. 9.42. It is now quite unable to move any part of its body. Died at 9.45.

Examination immediately. Great congestion of liver, kidney, portal vein, and substance of the heart. Right auricle engorged. The right auricle was seen to make about two slight contractions every minute, producing a wave in the blood, filling the vena cava descendens. These contractions were spontaneous; but after they ceased they could be excited again by mechanical irritation of the muscular substance for about half an hour after the organ was exposed.

Experiments on Frogs.

The experiments made on frogs were very numerous, and every experiment repeated frequently by my assistant, Andrianaly. Two species of frog were used, the one very considerably smaller than the common English frog, *Rana Temporaria;* the other about one-third larger. I shall select a few experiments from my note-book to illustrate some of the points referred to in the preceding observations.

Exp. IV. *To ascertain the effect of the local application of the poison.*—A little of the common extract was applied to the right leg of a frog at 9.20 A.M. At 9.58, the right leg is observed to be paralyzed, pinching it does not produce any sign of sensation, the left limb normal. 10.30. The left limb is now paralyzed. 10.40. The paralysis has extended to the whole body, and is gradually becoming more marked. The animal died about 11.15.

The two following experiments illustrate the action of *Tanghinia* on the heart.

Exp. V. At 9 A.M., exposed the heart of a large frog, the pulsations were about 52 per minute. 9.5. Injected about one grain of

the extract of *Tanghinia ven.* into the peritoneum. 9.8. Pulsations 52. 9.15. They became reduced to 39. At 9.20, the tetanized ventricle was seen to contract imperfectly, and at 9.21 the heart and respiration ceased almost simultaneously; but although the heart has ceased to beat, the animal made a few leaps. On looking at the posterior lymphatic hearts, we find they have ceased to beat. 9.28. The animal still continues to withdraw its legs if they are drawn out.

Exp. VI. Having exposed the heart of a large frog, and found the pulsations 42 per minute, at 7.15 we administered one grain of the extract of *Tanghinia v.* by the mouth. The action of the poison on the heart was as follows :—

7.20.	The pulsations	were	34.
7.25.	„	„	29.
7.30.	„	„	28.
7.40.	„	„	26, and irregular.
7.50.	„	„	24. The ventricle contracts very imperfectly.
8.	„	„	22. Slight vermicular motions of the ventricle.

8.10. The ventricle has ceased to beat, the auricles however make 12 pulsations per minute. 8.20. The auricles have stopped; but respiration has not quite ceased, and the animal is still able to move. The head was now removed, leaving the lower jaw connected with the body. Reflex movements could be induced by pinching, or by the application of electricity. The ventricle was contracted, and the auricles dilated.

Exp. VII. We selected two large frogs of about the same size, and having exposed their hearts, we administered two grains of the extract of *Tanghinia v.* by the mouth to one of them; and in order to test the action of the poison on the reflex functions of the cord, suspended them by the lower jaw, and observed the length of time which elapsed between the application of dilute sulphuric acid and the appearance of the corresponding reflex movements. We found the reflex movements to be less energetic, and slower in appearing in the poisoned frog; and after about an hour, scarcely any irritation could induce reflex movements; yet after death we found the muscles to contract, when magneto-electricity was applied, although less actively, and for a shorter time than the muscles of the non-poisoned frog. We observed in this same experiment paralysis of the lower jaw, which appeared almost immediately after the administration of the poison, and was probably owing to its topical action on the muscles and nerves. The pupils became much dilated within ten minutes after the poison was given, and continued at least double the size of the pupils of the non-poisoned frog. The posterior lymphatic hearts were also observed in this experiment, inasmuch as they receive their nervous supply from the cord. In the non-poisoned frog, the posterior lym-

phatic hearts continued to beat throughout the experiment at from 48 to 50 per minute, and the pulsations were *distinct*, *strong*, and *regular*. In the poisoned frog, they very soon rose in number: ten minutes after the administration of the poison they had risen to 63; after half an hour they were 77, and weak, and they gradually became weaker and more irregular, and after the lapse of an hour were quite imperceptible.

There is very great difficulty in deciding to what extent the conductivity of the motor nerves is affected, inasmuch as the poison affects the contractility of the muscles, and this complicates the matter very considerably. We constantly observe voluntary motion remarkably affected before the reflex functions of the cord are much diminished, but this loss of voluntary motion may of course be due to other causes than the mere loss of motor conductivity. Although I do not consider it proved, I think it probable that the helplessness and dragging of the limbs are partly owing to poisoning of the muscular tissue, and partly to paralysis of the motor nerves. It seems impossible to doubt that the irritability of the nerves and contractility of the muscles are lessened, and they are thus less able to respond to the influence of volition, while, on the other hand, this manifestation of reflex movements in the decapitated animal poisoned by *Tanghinia* proves that the muscles nevertheless retain enough of irritability, and the nerves enough of conductivity, to make it impossible to ascribe the paralysis entirely to these two causes. Admitting then these conditions as existing, I am inclined to think that the anterior columns of the spinal cord are also implicated, and that the paralysis, as I have already said, is partly due to this cause.

Exp. VIII. We decapitated a small frog in order to test the length of time that reflex movements may be excited by mechanical and chemical stimuli. 9.11. The frog was beheaded. 9.16. It draws up its hind legs if disturbed. 9.18. Makes attempts at leaping. 9.22. Pinching the foot produces movements in all the extremities. 9.31. Applied some dilute acid to the skin, and strong reflex movements followed. 9.40. Reflex movements cannot be any longer induced. In this experiment reflex movements persisted for nearly half an hour. A frog of similar size had its heart exposed, and a grain of *Tanghinia* injected beneath the skin of left leg. The pulsations of the heart rose from 80 to 100, and in two minutes stopped. The posterior lymphatic hearts stopped at the same time as the blood heart. We now cut off its head. Touching the eye induced pro-

tective motions of the eyelids. Pinching the feet did not bring on any reflex motion, but acetic acid applied to the fore-legs did. *Six minutes after the heart ceased to beat, all reflex action ceased.*

In repeated experiments in which the amputated limbs of frogs have been immersed in a mixture of *Tanghinia* the contractility of the muscles has been lessened in a very few minutes, and after a somewhat longer time abolished.

Exp. IX. We selected a large active frog, and tied the vessels of the left posterior extremity, and then poisoned it with extract of *Tanghinia* given by the mouth. The muscles of the limb so protected were found to contract much more energetically than those of the other, and that whether the stimulus was applied through the sciatic nerve or to the muscular substance itself.

Several experiments were made to ascertain whether destruction of the medulla would affect the action of the poison on the heart. In one case in which we decapitated a frog, and then injected one grain of the extract into the peritoneum, the heart continued to contract for twelve minutes after the injection of the poison, and dilute sulphuric acid induced reflex movements for some time after the heart was arrested. We may conclude that *Tanghinia* does not arrest the heart through any action on or through the vagus.

General Conclusions.

(*a*) The Tangèna must be classed among the cardiac poisons. It uniformly causes death by arresting the action of the heart.

(*b*) It does not act on the heart through the vagus nerve. When applied to the exposed heart its rapidity of action is remarkable. The fact that it arrests the pulsations of the excised heart of the frog is conclusive proof that its influence, when topically applied, is direct, either on the muscular substance, or the muscular substance *and* cardiac ganglia.

(*c*) There is sufficient reason to believe that the Tangèna acts on the spinal cord, producing paralysis and diminishing reflex action.

(*d*) Voluntary motion is abolished, and the irritability of the motor nerves lessened by the poison. When it acts through

the circulation in mammalia, sensation is not remarkably affected; muscular contractility is very much diminished. More exact knowledge of the degree and order in which these various functions are affected, can only be obtained by carefully performed experiments made in Europe, where the more delicate electrical instruments can be had.

(*e*) It is exceedingly fatal to man, in doses of thirty grains of the kernel, if not promptly ejected.

(*f*) It causes a numb, tingling sensation in the part with which it comes into contact, and also throughout the body.

(*g*) It is powerfully emetic and purgative, produces great nausea and debility, paralysis of motion, occasionally delirium, narcotism, and perhaps vertigo.

(*h*) It may be inferred to cause death in man, as in all other animals, by tetanizing the heart.

NOTE by Dr Galabin, *in continuation of his Paper* (p. 22).

Yet another has been added to the already numerous theories of the dicrotic wave by Mr Mahomed, in a paper lately published in the *Medical Times*. It is the more deserving of notice since it is to the able researches of its author that we owe the most recent contributions to our knowledge as to the clinical use of the sphygmograph. Mr Mahomed then finds a sufficient cause for the dicrotic wave in the mere fact that the coat of the aorta is elastic, and considers that it originates from the contraction of this elastic coat during diastole. It appears to me that this theory involves a misconception of the nature of elasticity. If a surface is said to be elastic, nothing more is meant than that it is extensible in such a way that the degree of its extension has a definite relation to the forces extending it. Its contraction is not an active proceeding, but is merely the effect of the diminution of tension. Thus the contraction of the aorta is the consequence of the diminished pressure within, and therefore the mere fact that it contracts cannot at the same time be a cause of an increase of that pressure originating a second wave.

CASE OF SUBDIVISION OF THE SCAPHOID CARPAL BONE. By JOHN STRUTHERS, M.D., *Professor of Anatomy in the University of Aberdeen.*

THIS condition was found in the right hand of a male subject, aged 68. My attention was called to it just after the ligaments of the first carpal row had been divided. The scaphoid is represented by two bones of nearly equal size. The lower or radial division carries about a fourth part, in length, of the articular surface for the radius, less than half of the articular hollow for the os magnum, and the whole of the surface for the trapezium and trapezoid bones. The direction of the articulation between the two bones is nearly at right angles to the general axis of the scaphoid. The surfaces are nearly flat from above downwards (i.e. in the direction between the surfaces for the radius and the magnum) allowing free gliding motion of the two bones on each other in that direction, but in the opposite direction motion is impeded by a little undulation of the surfaces. This articulation between the two bones occupies the entire thickness of each, and is continuous across the whole breadth with the synovial cavities of the surfaces for the radius and magnum.

So far the case looked like one of natural variation, but I was led to doubt this by the following considerations. The articular surfaces between the two bones, though polished and synovial looking, are hard, the knife turning up no articular cartilage till it reaches the edge of the other articular surfaces; and there are irregular depressions on parts of the polished surface, more distinctly seen when examined with a magnifying power. The trapezoid and os magnum, and the second and third metacarpals, where they meet at the carpo-metacarpal articulation, shew some bony excrescence on the dorsal aspect; and the shafts of the second and third metacarpals, especially that of the second, are altered above their middle, being elevated on the dorsal surface where they begin to be left uncovered by the dorsal interossei muscles, and having an

unusually projecting border on the palmar aspect. These two metacarpals, and the carpus when they join it, appear to have suffered some injury and to have undergone subsequent change. The scaphoid bone is naturally so placed that it would receive the shock of an injury transmitted from these two metacarpals through the moving trapezoid and magnum, and would be most likely to give way about the middle where it presents a kind of constriction or neck. My inference is that this has happened here, and that this case is not one of natural variation, but one of fracture of the scaphoid followed by the formation of a false joint. It might, however, readily have been mistaken for a case of variation, especially if the carpus had been already macerated, and the attention of observers may be called to the importance of looking narrowly to see whether some of these cases of additional carpal or tarsal ossicle have not had their origin in fracture. It should be added, that in this subject the left hand and both feet were normal and healthy.

ACCOUNT OF RUDIMENTARY FINGER MUSCLES FOUND IN A TOOTHED WHALE (HYPEROODON BIDENS). By JOHN STRUTHERS, M.D., *Professor of Anatomy in the University of Aberdeen.*

IT has been supposed that muscles passing from the forearm to the hand do not exist in any of the Delphinoid Cetacea. In a foot-note on page 115 of my paper "On some points in the Anatomy of a Great Fin-Whale" (Balænoptera musculus), published in this *Journal* for November 1871, I noticed briefly that I had found that such muscles are present in Hyperoodon bidens, a specimen of which had just been stranded on our coast. Having since dissected them more fully, and examined them also in the other paddle, I am now able to give an account of their arrangement. In order to save repetition, I shall take the muscles in the great Finner as a standard for comparison, and shall note the differences between them and the correspond-

ing muscles in Hyperoodon; and beg to refer to my detailed account, and to the drawings given, of these muscles in the paper above referred to.

These finger muscles, so far from being absent, are nearly all better developed in this toothed whale than in the great Finner.

(a) *Internal, or palmar, aspect.* 1. *Flexor carpi ulnaris.* The differences in this muscle are that implied in the different form of the cartilaginous olecranon, and that the tendon is flat and not placed so far from the shaft of the ulna. The cartilaginous olecranon is long, tapering, and curved backwards, and the origin of the muscle is continued to the point, running along the concave edge and some way on the inner surface of the cartilage. The upper convex edge of the cartilage gives insertion to the greater head of the triceps, though not back to the point. A muscular expansion from the trunk is inserted into the cartilage at its point and for some way along the inner aspect of the insertion of the triceps. The cartilage is very flexible inwards and outwards, from its flatness, and will be raised a little by the action of the triceps and depressed a little by the flexor carpi ulnaris. The insertion, as in the Finner, is entirely into the pisiform cartilage. This cartilage moves freely laterally, but has very little longitudinal mobility. It articulates distally with the fifth metacarpal bone as well as with the epiphysis of the ulna and the neighbouring carpal bone. The presence of this muscle cannot be satisfactorily accounted for on the theory of action upon its insertion or upon its origin.

2. *Flexor digitorum ulnaris.* The chief differences in this muscle are, that it is considerably more developed than in the Finner, that it lies more upon the ulna without being sunk into the interosseous hollow, and that the tendon retains its thickness instead of expanding before it divides. The fleshy fibres of origin reach farther forwards on the humerus, as far as to touch and even to pass a little under cover of the teres major; and further across the ulna so as to reach the flexor carpi ulnaris, the two bellies being in contact by their edges, the nerve disappearing between them. The belly lies upon the

proximal half of the ulna, the tendon obliquely across the distal half. The breaking up is just as it is getting upon the first carpal row, and is into three tendons. The ulnar tendon, after a course of an inch, divides for digits IV. and V.; the radial tendon, after nearly an inch, is joined by the tendon of the radial flexor.

3. *Flexor digitorum radialis.* This muscle, though less developed in Hyperoodon compared with the last muscle, is as well developed in proportion to the limb as it is in the Finner. Its fibres arise not only from the radius and interosseous tissue, but from the ulna fully as much as from the radius. It is sunk in the interosseous space. The tendon throws itself entirely into the radial tendon of the flexor ulnaris.

The *tendons* of these two muscles are disposed thus. The radial tendon, one-third of which is formed by the entire tendon of the radial flexor, is fully twice the size of the tendon of any other digit. It goes almost entirely to digit II., a few fibres going off to blend with the terminal ligament which connects the end of digit I. to the edge of the second bone of digit II. Its earliest insertion is into the metacarpo-phalangeal cartilage and broadly into the first phalanx, these insertions maintaining the previous obliquity of the tendon. Digit II., though but little longer than digit III., is the most robust of the digits. The middle tendon goes straight on to digit III. The radial subdivision of the ulnar tendon goes on to digit IV., the ulnar subdivision, the larger of the two, goes very obliquely towards digit V., and is broadly inserted into the first phalanx, which is mainly cartilaginous. The obliquity is maintained by this earlier part of the insertion.

(b) *Extensor, or dorsal, aspect.* In the Finner there is simply one common extensor, giving a tendon to each of the four digits. Here there is at least one other extensor, and, proportionately to the limb, a considerably greater bulk of muscle. There is the general division of the mass into a radial and an ulnar extensor, the latter somewhat complex. The fleshy fibres of both arise as high as the ligament of the elbow. The fleshy bellies occupy the proximal two-thirds of the fore-arm. That of the radial flexor reaches farthest back, is larger

by a third than the other, arises by its distal portion from the ulna as well as from the radius and interosseous tissue, and is mostly sunk into the interosseous hollow. The ulnar belly arises from the ulna and interosseous tissue, and both bellies have fibres from an intermuscular septum between them, and from a strong fascia over them.

The tendon of the radial extensor divides opposite the first carpal row into two, one straight on to digit III., the other, the larger, to digit II. The latter gives from its radial side a slip as if for digit I., but it is inserted into the distal part of the first joint of digit II., very nearly reaching the terminal ligament of digit I.

The ulnar extensor divides above the middle of the forearm into two fleshy bundles, from which tendons soon proceed, the most ulnar tendon beginning first. So far, these might be looked on as representing separate muscles, if the tendons bore out that view. The tendon of the radial portion passes to digit IV., adhering to the carpus on its way. The tendon of the ulnar portion after giving off a slip, one-third of its bulk, which joins the tendon of digit IV., opposite the second carpal row, passes on to digit V., and is inserted into the distal end of the metacarpal bone and into the phalanx beyond.

These details apply to both sides, but in the right paddle there is an intermediate fleshy slip, leaving the ulnar side of the radial flexor, soon ending in a tendon which divides at the carpus into two (adhering to the back of the carpus on its way), one joining the tendon of digit III., the other joining that of digit IV. The radial extensor thus, on this side, sends a tendon to digit IV. as well as to III. and II. A farther want of symmetry is seen in the presence, also on this side, of a small tendinous slip from the radial portion of the ulnar flexor to join the slip which the ulnar portion of that muscle gives to the tendon of digit IV.

Viewing these extensor muscles homologically, it might be held that the extensors of the carpus, at least on the ulnar side, are represented by the adhesions to the carpus and to the fifth metacarpal bone; but it may be sufficiently comprehensive to regard them as representing, the radial, the internal common extensor of the quadruped, the extensor communis digitorum

of man; and the ulnar, as representing the external common extensor of the quadruped, the extensor minimi digiti of man. The difference on the two sides illustrates the tendency to variation in rudimentary structures.

The extensors on the whole appear to be somewhat less powerful than the flexors, but the difference between the flexor and extensor powers is not so great as in the Finner. In accordance with the greater development of these muscles in Hyperoodon, the finger-joints are more moveable than in the Finner, the cavity reaching completely across between the cartilages, but the surfaces are quite flat. The same remark applies to the carpo-metacarpal joints and to those of the carpal cartilages, but to a less extent, the mobility at the joints increasing distally. To the merely passive resistance which a purely ligamentous condition would afford, these muscles will add some activity; but, though somewhat stronger than in the Finner, they are small and feeble relatively to the size and condition of the parts on which they act; in striking contrast with their fully developed neighbours, the muscles of the shoulder.

Rudimentary Brachial Muscles in Hyperoodon. In both paddles alike there is a short thick muscular and fibrous mass lying, like a cushion, along the lower border of the distal half of the humerus. It begins on the humerus just beyond the deltoid elevation, and terminates just beyond the elbow on the lower edge of the radius. It is about as thick as a fore-finger, flat next the humerus, rounded on the opposite aspect. It is largely mixed with fibrous tissue. There is some slight lateral mobility at the elbow, but none in the direction in which this muscle would act. Along the opposite border of the humerus lies a rudiment of the external short head of the triceps extensor cubiti muscle, passing from the border of the humerus to the early part of the olecranon. It is about as thick as a finger, 1½ inch in length, and very largely composed of fibrous tissue. The nerve and artery are seen to pass between it and the humerus, from the flexor aspect, and some tissue situated below this passage may be considered a remnant of the third head of the triceps. Under the microscope the reddish tissue

contained in these rudimentary brachial muscles is seen to be composed of well-striped good-sized muscular fibre.

The scapular head of the triceps is reinforced by a bundle from the teres, forming the distal part of the insertion into the olecranon. The limb having been removed from the trunk, it is now impossible to decide how far the latissimus dorsi may form part of what I have termed the teres major.

Size, age, sex, &c. This Hyperoodon was stranded, alive, at Fraserburgh, on the Aberdeenshire coast, on August 17, 1871. Length, along the curve of the back, 20 feet 9 inches; along the side, in contact, 19 feet 9 in.; in a straight line, 19 feet 3 in. Measurements of the paddle, in inches—length, from tip to axilla, 15; from tip to shoulder joint, 25, of which the humerus has 7½, the forearm 6½, the carpus 2, and the longest digit 9. Breadth, at axilla 6½, at broadest part 7. Girth, at axilla 15, at broadest part 15½. Compared with the great Finner (B. musculus), the paddle is shorter in Hyperoodon in proportion to the size of the animal; and comparing the segments of the respective paddles, Hyperoodon has, in proportion, a longer humerus, a considerably shorter forearm, and fingers of very nearly the same length. It was believed, when lying on the beach, to be a male, but circumstances prevented me from seeing the viscera afterwards. The maxillary crests are about five inches apart at the middle, and about three inches thick. The epiphyses of the bodies of the moveable vertebræ are all separate. The rudimentary teeth, one on each side, near the front of the lower jaw, are seen, in the preparation which I have preserved, to be buried half an inch below the surface of the dense gum.

TISSUE METABOLISM, OR THE ARTIFICIAL INDUCTION OF STRUCTURAL CHANGES IN LIVING ORGANISMS. By W. AINSLIE HOLLIS, M.D., Cantab., Part III., *Annulata continued.*

IN two previous papers I have given the results of some experiments made by me upon actiniæ and lumbrici; and I have there shewn that the tissues of such creatures are readily acted upon by caustic irritants (such as blistering fluid, strong acetic acid, &c.), and that a definite sequence of phenomena is thereby induced, which, in many respects, is similar for these two classes of animals. I shall in the following pages give a short résumé of some further experiments undertaken with a view to extend our knowledge of 'tissue metabolism' to other branches of the animal kingdom than those mentioned above. The first observations were made by me upon the medicinal leech (*H. medicinalis*), and are detailed below.

I. *The action of certain irritants upon leeches.*

Blistering fluid and acetic acid appear to act similarly when they are applied to the integuments of leeches. Either of these vesicants induces, immediately after its application, great swelling of the tissues, and a greater or less loss of the power of contraction at the injured part according to the extent of the injury. These phenomena are quickly followed by a copious secretion of mucus of a bluish white colour, and by the infiltration of the tissues with blood-plasma. In the course of an hour or less ecchymoses with intense congestion of the superficial blood-vessels are observable. The swelling of the parts with the vascular congestion usually lasts for twenty-four hours, subsequently contraction of the injured tissues takes place with puckering and the permanent loss of their contractility. During the second stage of symptoms detailed above, and before their final contraction, the tissues become softer than natural and are more easily torn. The mucous fluid exuded during the second stage consists of a viscid

hyaline plasma with numerous corpuscular, granular and pigmentary elements intermingled with it. The first of these bodies are about the size of human leucocytes, of oval shape (occasionally almost fibrillar); and they possibly possess amœboid movements. In two experiments, wherein blistering fluid was used, a distinct bulla was produced at the point of vesication; in one case this blister was filled with a colourless serum, in the second with a fluid of a reddish colour, and containing the corpuscular elements above described.

If we compare the above phenomena with those observable under like conditions in the structures of the actiniæ and lumbrici, we shall find a considerable similarity in them all, a resemblance which leads me to conclude that the processes in each instance are almost identical. I shall not at this stage recapitulate the various phenomena noticeable in these different animals, as they have been elsewhere detailed, and I shall have occasion to refer to them again at a future period.

II. *The application of the actual cautery to a leech.*

When a part of the dorsal surface of a leech is placed momentarily in contact with the end of a glass rod heated to redness, the burn instantly becomes of a bluish white colour and its surface is depressed. About ten minutes after the injury is made, the surrounding parts begin to swell, and subsequently mucus to a small extent is exuded from them. Within twenty-four hours the burnt tissues are covered with a shreddy bluish white débris, easily detached, and leaving the surface much depressed. The surrounding tissues at the same time become puckered and drawn towards the seat of injury. No bulla appears to be formed in this case; nor is there apparently any great exudation of serous fluid. It is necessary to notice these particulars, as I shall hereafter refer again to them.

Arthropoda and Mollusca.

My experiments upon several species of these two sub-kingdoms have been attended with no marked results. I have on various occasions tried the effect of irritants and the actual cautery upon members of the class *arachnida;* but, owing to the thickness of their chitinous integument, the ordinary

irritants appeared to have little effect upon them, whilst more powerful corrosives or the actual cautery destroyed life without inducing any of the phenomena noticed in the other classes of animals. Like results have followed my attempts to induce metabolic processes in various insects. The larvæ of the lepidoptera (upon which most of my experiments were performed), although manifesting by their movements that blistering fluid, nitric acid and other irritants were local sources of uneasiness to them when applied to their chitinous investment, failed entirely to manifest any of those palpable changes of structure noticed by me in the annulata, although they sometimes rapidly died after the operation.

Slugs, snails and other gasteropods, upon which I have experimented as in the preceding cases, appear to a great extent to be protected from such possible sources of injury by the rapidity with which a large quantity of mucus is evolved from the affected surface. This flow of mucus carries away with it the local source of irritation. When the actual cautery was applied for a moment to the foot of a snail a like evolution of mucus took place at the point of contact, and a whitish eschar was visible; but on the following day this local change had been removed with a large quantity of mucus, and was entirely separated from the animal, whilst the surface of the mollusc appeared in all respects natural.

VERTEBRATA. AMPHIBIA.

In dealing with the subject of 'tissue metabolism' among the higher animals, it will be necessary to encroach somewhat upon the labours of others in a similar direction, notably on those of Ryneck, Stricker and Cohnheim, in their researches upon the process of inflammation. I trust, however, that the importance of the subject, and a certain novelty in the method of performing the experiments, may plead an excuse for pursuing a path already trodden by such able physiologists.

The experiments I shall now detail were performed upon the common smooth newt (*Lissotriton punctatus*).

I. *The application of certain irritants to the abscised tail of a newt.* When the tail of a newt is freshly removed from the

body it will under favourable conditions continue to give signs of life by occasional movements for eight-and-forty hours and upwards. It therefore appeared to me to be a favourable object upon which to pursue my experiments on 'tissue metabolism.' With this view I applied blistering fluid to the recently abscised tail from a decapitated newt, and set it aside in a small quantity of water for a few minutes. The part vesicated was then of a bluish white colour and covered with a copious exudation of mucus. In two hours time the portion of the tail untouched by the Liquor Epispasticus contracted readily on the application of a slight stimulus, while the rest of it remained uncontracted. Twenty-four hours subsequently several bullæ were observable on the injured portion, and these contained a fluid and a minutely corpuscular plasma. The cutis was easily detached from the subjacent structures at the seat of injury, and these were found to be swollen and infiltrated with a watery plasma containing numerous granules and a few leucocytes. I have since repeated this experiment with similar results. If all movement has ceased in the tail before the application of the blistering fluid, beyond the whitening of the integument, no further phenomenon takes place. This I think clearly shews that the exudation of plasma and the other concurrent symptoms are due to the active vital properties of the tissues. Now the structures in a newt's tail after its removal from the body appear to me to approach closely in their constitution to the whole body of those invertebrate annelids we have lately been considering, with this difference in favour of the greater heterogeneity of the latter, namely, that they possess a definite circulatory fluid driven through their tissues by appropriate pulsatile organs, whilst the detached caudal extremity of the newt has no such provision for its general tissue nutriment. In such cases the various phenomena observed after irritation must, in the first place, I think, be considered entirely due to the local action of the irritant on the individual living elements of a part; for it is difficult to suppose that either the nerves or blood-vessels, owing to the absence of the blood, can exert any more than a very slight influence over the tissue-changes that take place. If at the same time that an irritant is applied to the abscised tail of a

newt a similar injury is induced on the tail of a healthy newt, the various phenomena detailed above will correspond in each case—although in the latter the secondary effects on the blood-circulation will be evidenced in the well-known phenomena accompanying its stasis.

II. *The effects of a burn on the abscised tail of a newt.* When the end of a red-hot glass rod is momentarily applied to the integuments of a recently abscised tail the burn becomes depressed and whitish, while the adjacent tissues are rapidly elevated and covered with a bluish white débris—as a rule no bullæ are formed in such a case—and this corresponds with what I have observed to take place in leeches. In twenty-four hours after the injury the burnt surface is covered with a ragged epithelial and granular débris and the surface is still much depressed; the surrounding tissues have usually resumed their natural shape, and no further phenomena are observable. This cessation of further changes in the part is probably due to the greater shock of the cautery to the tissues themselves, and the more rapid death of the tail generally than is the case when the blistering fluid is used.

III. *The application of blistering fluid to the excised heart of a newt.* Upon touching the ventricular apex of a heart still pulsating, and freshly removed from the body of a newt, with about $\frac{1}{100}$th of a drop of Liquor Epispasticus, contained in a capillary tube, there was an immediate pallor and loss of contractility in the part, and subsequently a slight swelling. The loss of contractile power was very manifest in this case, as the seat of injury was sharply defined by the change in colour. The remainder of the organ continued its usually rhythmical pulsations, whilst the pallid portion was uncontracted after the application of the vesicant.

General Remarks. If we exclude the arthropoda, and in some cases the molluscs, from these observations for reasons elsewhere stated, these experiments tend to shew that there is a general correspondence throughout the animal kingdom in the sequence of phenomena observable after the application of an irritant to a living surface. These may be briefly summarised as below:

1. Swelling and loss of contractility. 2. Exudation of fluid (muciform or serous). 3. Elevation of the external integument with the production of a bulla, containing corpuscular elements of various shapes. (This phenomenon is only observable where there is a distinct epiderm, and occasionally not then.) 4. Infiltration of the subjacent tissues with fluid plasma and their subsequent disintegration, when the cause of irritation is prolonged. 5. Subsequent contraction of the subtegumentary tissues and permanent loss of their contractility, should the injury extend to them.

What then do these phenomena imply? Dr Ryneck[1] has shewn—by first removing the blood from the vessels in the web of a frog and "then subjecting their internal surfaces for a few moments to an agent, which, by virtue of its chemical action, might be expected to modify or destroy its vitality; and finally, after replacing the injurious liquid by milk or defibrinated blood, observing the effects of local irritation"—that no stasis is produced in webs which were thus treated, while the phenomena of stasis occurred in vessels after local irritation, even when their natural contents were replaced by milk. Such "results seem to make it perfectly clear that the local changes which lead to the production of stasis must have their seat either in the walls of the vessels or in the tissues which immediately surround them."

Upwards of twenty years ago H. Weber[2] shewed that even after ligature of the thigh of a frog the blood gathers from all sides to the irritated part of the web, when ammonia is applied to it; and quite recently Cohnheim[3] has shewn that the opinions of Hering and Schlarewsky, who ascribed "the phenomenon of extravasation to the slow filtration of a colloid substance through the walls of the vessels," is incorrect, by placing an animal in the required conditions for physical transudation, when none of the phenomena which he (Cohnheim) has described elsewhere will take place. "Not a single

[1] Zur Kenntniss d. Stase des Blutes in d. Gefässen entzündeter Theile, in Rollett's *Unters. a. d. Instit. f. Phys. u. Hist. in Graz.* 1870, p. 103, noticed in Holmes' *System of Surgery*, 2 ed. v. p. 757. (The process of inflammation by Sanderson.)

[2] Müller's *Arch.*, 1852, p. 361. (Holmes, *Op. Cit.* v. p. 763.)

[3] See article in *Brit. Med. Journ.* Sept. 27, 1873, on Cohnheim's paper, *Neue Untersuch. ü. die Entzündung*, Berlin, 1873.

red or white corpuscle passes through the walls of the vessels, in spite of the walls of the veins being lined with white corpuscles, and of the increased blood-pressure in the capillaries." In summing up the results of his observation, Cohnheim believes that the essence of inflammation consists in some local change in the walls of the vessels of the affected part, by which the extravasation of the corpuscular elements of the blood can take place. What this change is, he is however unable to say, as he has not succeeded in finding any structural alteration.

The results of my own observations lead me to believe that the effect of irritants of all kinds on living structures is to diminish or destroy their vitality, and with this their tonicity; in the case of the epiderm, the destruction of tissue leads to its removal from the body as a dead substance, and its subsequent renewal by the subjacent structures without modification of its original form. When, however, the deeper structures are involved in the destructive process, they are never replaced, except as lowly organised cicatricial tissue. This tissue never inherits the voluntary contractile power of the muscle it frequently represents, nor does it actually equal in volume the original structures, for a permanent depression always marks the place of injury.

It seems that we must look upon the "essence of inflammation" to be a weakening or loss of vitality in the tissue-web of a part, and that the changes which take place in the capillaries, their dilatation and the permeability of their coats, are due to this loss of tonicity in their walls, and herein their parietal elements only share with the muscles and the nerves (if any) the same general change in their conditions, which produces on the one hand loss of contractile power, on the other loss of sensation.

If we enquire the prime cause of these functional changes, I think it will hereafter be found to be due to a molecular change in the tissue-elements induced by the modified conditions of their environment, whether such modifications be brought about by mechanical, chemical, or purely physical methods; but to this question we have at present no means of replying satisfactorily.

ON THE MECHANISM OF OPENING AND CLOSING THE EUSTACHIAN TUBE. By C. J. F. YULE, B.A.,

Scholar of St John's College, Cambridge, and Fellow elect of Magdalen College, Oxford.

(*From the Physiological Laboratory in the University of Cambridge.*)

DR TOYNBEE was the first, I believe, to state the proposition that the Eustachian tube is normally closed, and that it is opened only during the action of swallowing. Up to his time the reverse was believed to be the case. The chief experiment which he adduced in support of his view was the old one of swallowing with the mouth and nostrils closed; this, as is easily felt, produces a distressed feeling in the ears, which he described as a "fulness or distension." He proceeds to say, "this sensation arises from the air which is slightly compressed in the fauces, passing into and distending the tympanic cavities; upon removing the hand from the nose it will be observed that the feeling of pressure in the ears does not disappear, but remains until the act of deglutition is again performed while the nose is not closed." This observation is very pertinent and valuable. Its value, moreover, is not lessened by the mistake into which Toynbee fell, as pointed out by Dr Jago, of supposing that in the experiment air was forced *into* the tympanic cavity, while it is in reality sucked *out*. The fact remains the same, namely, that to cause any flow of air out of the tube considerable force has to be used; and also that when an adequate effort is made, the action takes place suddenly, as if an obstacle to its flow had been burst open, and not gradually overcome, as would be the case if the tube were patent. Now the tympanic membrane is very sensitive to distension, as anybody may convince himself by blowing or sucking ever so slightly, while the Eustachian tube is kept patent with a catheter adapted for the purpose. It cannot, therefore, be objected to Toynbee's experiment that the air is all the time passing out through a patent tube, but that the unpleasant sensations

are only felt when the pressure on the tympanic membrane reaches a certain maximum, and that the attainment of this maximum is mistaken for the opening of the tube. Toynbee also describes the inverse experiment, where the pressure on the outside of the tympanic membrane is greater than within it, as during a descent in a diving bell. In both these experiments the inequality of pressure is removed by swallowing, owing to the fact that the muscles which open the Eustachian tube are also implicated in swallowing; and, moreover, that after the effort of swallowing the closure of the tube takes place without effort. If a slight modification of the first experiment be adopted, and air be blown into the tympanum instead of sucked out, considerable pressure can be resisted by the Eustachian tube before the air finds a passage. The amount of this resistance I have endeavoured to ascertain by various methods—first, the injection of fluids; this was performed upon a patient suffering from a large perforation of the tympanic membrane, but possessing at the same time a healthy Eustachian tube. The apparatus used was an ordinary ear-syringe with T-piece and manometer with mercury in the bend, introduced before the nozzle. The results obtained were nearly valueless. A dilute solution of potassium carbonate passed freely at a pressure of four inches of mercury, while a solution of alum so acted upon the tube as to constrict it and prevent the passage of all fluids. After injecting the potassium carbonate solution, blowing into the ear failed to force a passage, although the pressure was raised as high as seemed to be compatible with the safety of the fenestræ. At the same time that I was blowing, air passed into my own tympanum from the guttural orifice; so that this experiment may shew either that there is a valvular action in some part of the tube, allowing passage to take place more freely from the throat to the tympanum than in the reverse direction; or that there was some unsuspected disease of the patient operated on. Of these two alternatives I think the former is the more probable, because I find it certainly easier to distend than to exhaust the tympanum.

Finally, as a crucial experiment, to convince myself of the closure of the tube, I had a catheter passed into my own Eustachian tube, in order to compare the sensations felt in the

normal ear with those in the catheterized one. This was kindly done for me by Mr Hinton, to whom, for his courtesy and patience, I desire to express my thanks. I then found, as Dr Jago has suggested, that sounds produced in the larynx were rendered very much louder, indeed, so much so, that on humming a note just loud enough to be heard by a person standing close to me, the noise produced appeared as powerful as the loudest notes of my own voice; while by singing loud the noise was painfully intense, and closely resembled the sounds heard when a person puts his mouth near the external meatus and sings. The catheter used was an ordinary Eustachian tube gum one, with a hole cut in the outer side of the knee. As it was impossible on account of this hole to percuss the tympanum in the usual manner, several failures resulted, the open end of the catheter becoming blocked by mucus, but by passing a catgut bougie along the catheter, and for about a quarter of an inch beyond its terminal orifice, this obstacle was removed. My attention was first directed to the question of the opening and closure of the Eustachian tube by a faculty of which I had been long aware, that while singing I could, by an effort of some faucial muscles, cause the sound to appear very much louder, in a manner which I have compared to the "swell" arrangement of an organ. The increase in apparent loudness was very marked indeed. It naturally occurred to me that this was due to the opening of the Eustachian tube, and this idea was afterwards confirmed by the following experiments:—First, when the tympanic cavity was blown full of air and the membrane sensibly distended, the air which was retained by the natural closure of the Eustachian tube, at once escaped on the contraction of these muscles. Second, the effect of passing a catheter of the form before described into the Eustachian tube, was to produce a modification of hearing exactly identical in every respect with that observed when the contraction of these faucial muscles takes place.

That the Eustachian tube opens during the act of swallowing has been long admitted, but owing to the extreme complication of the act, no observer has yet been able to ascertain clearly which the efficient salpyngeal muscles are. By a careful examination, however, of my own pharynx during the opening

of the Eustachian tube, when not complicated by swallowing, their action is easily understood. In this matter Dr Durham of Guy's Hospital has been good enough to assist me, and I wish to express my obligation to him for his kindness and material assistance during the laryngoscopic examinations necessary. It is noticed during the contraction for opening the tube: First, that the velum palati does not change either its position or shape, in fact, that it remains unmoved; and further that it does not become tense, but hangs as soft and flaccid to the touch as at ordinary times of rest. Secondly, that the only parts which do move are the two posterior pillars of the pharynx; and their motion is ample and decided and altogether unmistakeable. They both move inwards simultaneously towards the middle line, moving from their old position from one-half to three-fourths of an inch. This action is not spasmodic, but perfectly steady, and can be sustained for some considerable time at will, the pillars maintaining their new position all the while.

Now I am quite satisfied and certain that during this period the Eustachian tube is open. It will be noted that from the flaccid condition of the velum, and also from the fact of its position and form remaining unaltered, the tensor and levator palati can have no participation in the opening of the tube, and that the muscles most evidently concerned are the palato-pharyngei.

Another point to which I wish to call attention is the sound which accompanies the opening of the Eustachian tube. It is a sharp crackling sound, which is referred to some part of the tympanum, or perhaps to the membrane itself. This, I have no doubt whatever, is caused by the separation of the walls of the Eustachian tube; I have not succeeded in imitating it in the dead human subject, but it can be very readily done in the sheep. By taking hold of the fold of mucous membrane, which half covers the lumen, and drawing it gradually inwards, the tube is opened, and at the moment of separation of its walls a sound is heard closely resembling that which I have described. The sound can be heard by anybody at the commencement of the act of swallowing, and forms the prelude to a series of noises arising from the moving of mucus, &c. about the pos-

terior nares and pharynx. I find it is most distinct when a bolus of tolerably dry food is swallowed.

On examining the anatomy of the parts, the appearances presented during the opening of the tube will be readily understood. The cartilaginous continuation of the Eustachian tube, or salpynx, presents at its upper part a massive lobe, to this is attached the tendon of the salpyngo-pharyngeus. The direction of this muscle from above is downwards and a little outwards when the palato-pharyngeus is at rest, and its action in this state would be to press the internal lobe of the salpynx inwards towards the orifice; thus assisting by its tonicity the elasticity of the cartilage in keeping the tube closed. The two muscles upon which the opening of the Eustachian tube depends are the salpyngo-pharyngeus and the palato-pharyngeus. The former muscle, as before explained, is attached above to the lobe of the salpynx by a tendon, and below its fibres mix with those of the palato-pharyngeus; it has a separate existence, however, for a distance of about an inch and a half. The palato-pharyngeus is divisible into two parts, a lower vertical and an upper curved one, arching inwards towards its fellow on the other side; when the muscle contracts it is evident that the curved part becomes straighter, and that the straight part is drawn inwards; and as the salpyngo-pharyngeus arises from the lower part its origin is also carried inwards. The effect of this is to give the action of the salpyngo-pharyngeus a new direction, such as to pull the lobe of the salpynx towards the middle line and out of the orifice of the Eustachian tube, thus opening the cavity of the tube. This is the rationale of the approximation of the posterior pillars of the pharynx.

Of the other muscles around this part of the pharynx nearly all have at various times been stated to have an opening or closing effect on the Eustachian tube. The tensor-palati passes backwards outwards and upwards past the inner edge of the salpynx, but does not seem to be attached to the salpynx, or to be attached in such a manner as to open the tube at all. Lower down lies the levator-palati. This muscle may perhaps have some little action in opening the tube when it contracts. Slightly above its middle point it is crossed by the salpyngo-

pharyngeus; and it possibly may assist, when its diameter is increased by contraction, in giving to the salpyngo-pharyngeus its new direction; but that this action is very insignificant is shown by the fact, that during the opening of the Eustachian tube, the velum-palati remains flaccid.

In the sheep the salpynx has a different form from that in man, the chief peculiarity being the absence of the bluff mass of cartilage on its inner edge. The free edge is composed of a thin flap or fold of connective tissue, covered by mucous membrane. On examining it from the inside, by making a longitudinal section of the head in the median plane, it will be seen that the free edge of the salpynx projects very little—scarcely at all above the surface of the mucous membrane. The orifice of the tube is closed, most distinctly so, and is covered by a layer of mucus. It is also to be noticed, that the free fold of mucous membrane passes directly downward to the posterior pillars of the fauces. Now the contraction of the palato-pharyngei draws this fold of mucous membrane inward and opens the tube. This action is easily imitated in the dead subject.

For assistance in the dissections I am much indebted to Dr Wilson of the Cambridge University school of Anatomy.

EXPLANATION OF PLATE.

Fig. 1. Cavity of the pharynx viewed from the right side in Man. A. Hard palate. B. Velum Palati. C. Azygos Uvulæ. D. Tensor Palati. E. Levator Palati. F. Palato-pharyngeus. G. Salpyngo-pharyngeus uniting below with the palato-pharyngeus. K. Lateral cartilaginous lobe of the Salpynx. L. Tendinous insertion of the salpyngo-pharyngeus.

Fig. 2. Anterior view of the pharynx during rest. A. Anterior pillars of the Fauces. B. Posterior pillars of the Fauces.

Fig. 3. Anterior view of the pharynx during the opening of the Eustachian Tube. Letters as in Fig. 2.

Fig. 4. Anterior view of the Eustachian Tube in the Sheep when closed.

Fig. 5. Ditto, when open.

NOTE ON A BIDENTAL SKULL OF A NARWHAL.
By PROFESSOR TURNER.

MR J. W. CLARK of Cambridge, in his excellent notes[1] on skulls of the Narwhal, in which two developed tusks have been found, refers to a statement made by Dr R. Brown, in his account of the Cetaceans of the Greenland Seas (*P. Z. S.* 1868, p. 353), who says, "Among other double-horned skulls which have been preserved, there is a fine specimen, presented by Captain Graville, in the Trinity House, Hull—one of the teeth is 3′ long, and the other 4′." Mr Clark, being desirous of obtaining fuller information respecting this specimen, wrote to the Curator of the Hull Literary and Philosophical Society, who replied that no skull of the Narwhal could be found in the Museum of the Trinity House, and on enquiring of a friend of the Gravilles, he could not hear that such a skull had been in their possession.

As I had some recollection of hearing my former pupil, Mr Charles Edward Smith, who acted as Surgeon to the whaling ship "Diana" on her disastrous voyage, when Captain Graville and others of her seamen died of cold and hunger, speak of a two-horned skull of a Narwhal in the possession of Captain Graville, I took the opportunity, as he was passing through Edinburgh in the month of July last, of asking him to give me some account of the specimen. Mr Smith very kindly complied with my request, and furnished me with the following note:—

"July, 1866. I went ashore to the Esquimaux settlement at Button Point, on the north side of Pond's Bay, to barter knives and ammunition for Narwhals' horns. The Esquimaux brought out of a pool of water the skull of a Narwhal with the two tusks projecting from the upper jaw, one was 7 ft. 4 in. long, the other 7 ft. 1 in., and the points were unbroken. The tusks were not parallel, but diverged from each other, so that a man could stand between them at their free ends. When Captain Graville saw this skull he claimed it as his perquisite;

[1] *Proc. Zool. Soc. of London*, Jan. 17, 1871.

and in the month of September, about three months prior to his death on board the 'Diana' from exhaustion, he cut the two tusks out of the skull. I may add that I was much distressed at witnessing the destruction of this specimen, and interposed in vain to save it."

There can, I think, be little doubt that two of the Narwhal tusks, which Mr Clark states were shown him by Mr Wareham, the dealer in curiosities, who had bought them out of the "Diana," and who had been informed by the mate "that two of them were taken out of the same skull," were the teeth obtained from that particular cranium, the destruction of which Mr Smith has given me an account of.

AN ABNORMAL ISCHIO-TROCHANTERIC LIGAMENT.

By THOMAS DWIGHT, Junr., M.D., of Boston, United States of America, *Professor of Anatomy at the Medical School of Maine.* (Plate VII., Fig. 1.)

THE anomaly was observed in February, 1873, on a male subject of large size and of remarkable muscular development. In the left hip a smooth and glistening tendon (A in figure) arose, above the tuberosity of the ischium, from the outer edge of the cartilaginous surface over which the tendon of the obturator internus plays, and ran outward to be inserted into the anterior part of the digital fossa. At its origin it was one-eighth of an inch distant from the capsule, and it remained distinct from it for the first half of its course. After joining the capsule the tendon expanded laterally, but without at all losing its individuality. Below this there was another band (B in figure), closely connected with the capsule, from which it received in its lower border a broad band of circular fibres. B is, no doubt, an uncommonly well-marked instance of Barkow's ischio-capsular ligament[1]; but the upper band (A), on account

[1] Vide Henle's *Bänderlehre.*

of its denser structure, its slighter connection with the capsule, and its want of union with any of the circular fibres, cannot be considered a similar hypertrophy, and admits, moreover, of a more plausible explanation. As there can be little question that in many positions of the leg the obturator internus plays the part of a ligament restraining the great trochanter; as further, in human anatomy at least, the gemelli are practically parts of this muscle, their edges usually meeting under cover of its tendon; and as the anomalous band arose close beneath the gemelli, was inserted near the obturator internus, and must have had a perfectly similar limiting function, it appears most natural to consider it a repetition of that muscle. After the drawing had been made, the joint was opened through the wall of the pelvis for the examination of the round ligament, which was found larger than usual.

The subject presented some other anomalies, all of which were on the side of excessive development. The clavicles were very strong and much curved; the sternal ends were prolonged backward as strong processes, and each bone had a true synovial articulation with the first rib. The left semi-tendinosus gave off a strong tensor fasciæ suralis.

DEPRESSIONS IN THE PARIETAL BONES OF AN ORANG AND IN MAN—SUPERNUMERARY MOLARS IN ORANG. By PROFESSOR HUMPHRY. (Plate VII. Figs. 2 to 6.)

FIG. 2 represents the skull of an adult female Orang from Borneo, lately presented by Mr W. Vores, of Caius College, to the Anatomical Museum of the University of Cambridge, which shews depressions on the exterior of the parietal bones similar to those occasionally found in the human skull, and to which I have directed attention in my *Human Skeleton*, p. 242. These depressions in the Orang are at the middle of the parietal bones, placed, almost symmetrically, on the sides and at a short distance from the sagittal suture. They look as if the bones had been indented when in a soft state by the pressure of two fingers; the deepest part, which is at the middle, is about a line below the level of the surrounding bone. The bone rises gradually to the circumference; and the whole surface is smooth like that of the remainder of the skull. The depression on the right side is irregularly oval, and is $1\frac{1}{8}$ inch in its longest diameter, which is parallel with the sagittal suture, and $\frac{5}{8}$ inch in its transverse diameter. That on the left side is more circular and about an inch in diameter. A quarter of an inch in front of this is another (a third) depression, more superficial and smaller, measuring $\frac{5}{8}$ inch by $\frac{3}{8}$, with the longest diameter antero-posterior. There is no corresponding alteration in the contour of the interior of the skull; and there is no other peculiarity in the skull except the additional molar teeth, presently to be mentioned.

Fig. 3 represents the calvarium of a human skull in the Cambridge Museum, in which the parietal depressions are very marked. They are ovoid, nearly symmetrical, slanting obliquely from the posterior and inner part of the parietals, a quarter of an inch from the sagittal suture and from its junction with the lambdoidal on the two sides, outwards and forwards, to about an inch from the coronal suture midway between the sagittal and the spheno-parietal sutures. That on the right

side measures 3⅝ inches in the longest or antero-posterior direction and 2 inches in its greatest transverse diameter. That on the left side is 2⅞ inches long and 1⅝ inch in its greatest width. They are smooth, though finely pitted, and somewhat uneven both in surface and contour. The edges are bevelled; and the deepest parts, which are about the middle, extend almost through the skull, leaving only a thin semi-transparent plate of the inner table. They look as if a portion of the outer table and diploe had been sliced off, leaving the inner table, which preserves its proper level. At the middle of the sagittal suture is a similar depression or indentation, 1⅛ inch by ⅝, involving of course both parietals. The interior of the skull presents no alteration in its contour corresponding with these external depressions.

Fig. 4 represents a transverse section through the parietal depressions and through the median sagittal depression. It does not indicate quite the thinnest part of the parietal bones. It shews that the inner table retains its proper contour. The bulging into the interior at the middle, which, it will be observed, does not quite correspond with the sagittal depression, has no relation to it, but is caused by the thickening of the skull in the vicinity of the median suture.

I have met with similar depressions in a few other instances in the human skull. They exist in the skull from an elderly woman in the museum of the College of Surgeons; and I recently found them in the parietal bones of a woman, æt. 73[1], who died suddenly of apoplexy, and in whom the skull, the calvarium especially and the frontal bones in particular, are thick and heavy from osseous deposit in the interior. The internal surface of the frontal bones is remarkably uneven or nodulated from this hard deposit, which would appear to have taken place recently, during the latter years at least of the patient's life, and which was probably associated with, if not dependent on, shrinking of the brain. The deposit has occurred upon the interior of the depressions as well as of other parts of the parietals, but does not appear to have taken place

[1] Dr Davis, in a note in the *Crania Britannica*, p. 6, mentions the skull of a very aged Chinese, in which the central area of the parietal bones is thinned and depressed, over an extent equal to four square inches, to about one-third of an inch deep in the central part. See also Paget's *Lectures on Pathology*, 3rd Ed. by Turner, p. 101; and Rokitansky, *Handbuch der Path. Anat.* II. 243.

upon the exterior of the skull. The specimen is in the Cambridge Museum. The depressions correspond generally with those in the specimen I have described; though the outline is rather more irregular, and the inner edge of each is $1\frac{1}{4}$ inch from the sagittal suture. There is also in this case a depression at the middle of the sagittal suture, though smaller and shallower than in the specimen last described.

I can offer no explanation of the occurrence of these parietal depressions or connect them with anything of embryological or morphological interest, and I have not met with them in any other animal besides this Orang and Man. They appear to be due to an imperfection in the ossifying processes in consequence of which this part of the skull is left thin, or, it may be, if the specimen of the Peruvian, presently to be mentioned, is an instance of the kind, perforated. They are not necessarily congenital. It may be that the thinness is due to a want of proper balance between the bone growth on the exterior of the skull and the bone absorption on the interior, which accompany and are caused by the enlargement of the brain, and which effect the requisite enlargement of the cranial cavity after the sutures are closed. But why the deficiency should especially manifest itself at this part, on either side, I cannot tell. Their position is not at the parietal protuberances, where ossification commences, or at the sagittal suture, where it terminates, or at the parietal foramina, which are sometimes preternaturally large. As we have seen, it is, in some instances, associated with similar thinness in the course of the sagittal suture; and there may be an approach to the same thing in other parts of the calvarium, when it does not exist in this region of the parietal bones. Thus in a skull in the Cambridge Museum there is a wide shallow groove in the situation of the hinder part of the sagittal suture, which is continued, on either side, in the course of the lambdoidal suture; and in the skull of a South Australian, in the same museum, there is a slight depression in the frontal bone, on either side of the median line, corresponding in position and appearance with those I have described in the parietal bones, but much less marked. Indeed I should not have noticed it had I not been close questioning the skulls in our museum in reference to this point.

These parietal depressions commonly exist on the two sides[1] and are symmetrical in position, and more or less so in form, size, and depth. They do not present any indications of being the result of disease. They are certainly not the result of accident. They may be caused by absorption of the outer tables of the skull. It is however as difficult to know why absorption should attack this region as why deficiency of formation should be manifested here. Pressure, interfering with formation of the bone or inducing its absorption, would cause such depressions. But whence the pressure? Clearly not from wens or other cysts or morbid growths; and I am not acquainted with any ordinary or extraordinary influence that would be likely to produce the effect.

The depressions have a practical bearing, forasmuch as they might lead to the supposition, after an accident, that there was depression of the bone, or they might be supposed to indicate that there had been loss of bone after accident or disease followed by imperfect reparation. This mistake seems to have been made in the case of the specimen in the museum of St Thomas' Hospital, referred to by me (*loc. cit.* p. 243). In the skull of an ancient Peruvian, in the Cambridge Museum, there is a slight depression in the usual position in the right parietal bone; and in the corresponding situation of the left parietal bone there is a large circular, uneven, slightly depressed space in which at one part the depression is deeper; and at two other parts in the same area the depression extends quite through the skull, giving rise to two holes with sharp edges and bevelled outer margins. The interior of skull presents, so far as I can see through the foramen magnum, no alteration, except the perforations. Now, in this case, it is extremely difficult to decide whether or not the superficial and the deeper depressions and foramina on the left side are congenital, as is probably the depression in the corresponding situation on the opposite side, or whether they are the result of accident or disease. Gall appears to have possessed "the skull of a full-grown man, whose exterior lamella of the os bregmatis on both sides had been

[1] In my treatise *On the Human Skeleton* I mention the case of an infant born with a depression in the right parietal bone, which I thought to be of the same nature as those I am describing; but the examination was made in the living child, and I could not therefore form any decided opinion respecting it.

broken by Levret's forceps, and had not been replaced in its former situation. The whole form of the forceps is said to be distinctly seen on the outside, but on the inside of the lamella not the least impression is discernible, and consequently had been restored to its due form through the action of the brain[1]." Doubtless, this was an instance of the depression in question; and one cannot be surprised at the supposition being entertained, that the depression was caused by the pressure of the forceps, for the appearance of the exterior (in the instances I have described) is suggestive of such a cause; though there is an absence of any corresponding projection in the interior of the skull.

In a paper in Virchow's *Archiv*, VII. 338, Dr Maier describes two specimens of this condition of the parietal bones, both from aged females, and regards it as the result of senile osteoporosis, the bone being yellowish, fatty, and porous, the porosity being, partly, due to enlargement and fusion of the Haversian canals. In neither of the specimens I have described is there any appearance of osteoporosis. The bone in the region of the depressions is as dense as it usually is, and as it is in other parts of the calvaria. And sections taken from the human specimen, represented in the figures, present, under the microscope, a normal appearance with corpuscles and Haversian canals of the usual size and appearance; and the bone structure shews nothing abnormal[2].

Another anomaly in this Orang's skull is presented by the presence of an additional, or sixth, molar tooth situated behind the usual series on either side of the upper jaw and on the left side of the lower jaw. These teeth, though smaller than the other molars, are well formed, having each four cusps and the normal complement of fangs, viz. one in front and one behind

[1] Dr F. Gall's *System of the Functions of the Brain*, extracted from C. A. Bode's account of Gall's lectures, p. 62.

[2] In Virchow's *Gesammelte Abhandlungen zur wissenschaftlichen Medicin*, S. 1000, is a paper über die Involutions-Krankheit (malum senile) der platten Knochen, namentlich des Schädels, in which specimens are described where the parietal depressions were accompanied with more or less porosity of the remainder of the roof of the skull and were associated, in one case, with thinning of the blades of the scapulæ and of the middle of the iliac bones, and, in one, with thickening of the skull. The condition is regarded as the result of atrophy commencing on the exterior.

in the lower jaw, and two on the outer and one on the inner side in the upper jaw. On the right side of the lower jaw there is no trace of an additional or sixth molar, though there is nearly as much space for it as on the other side. Instances of such additional molars in these animals have been seen by others; though it has not happened to myself to meet with one before. They are of interest not only in connection with the fact of the presence of an extra molar (a third premolar) in the monkeys of the new world, but also with the fact that in some of the old world monkeys, as Macacus and Colobus, there is a tendency to occupy the space which the elongated alveolus of the jaw presents by the formation in the lower dental series of an additional or fifth cusp at the hinder part of the last molar, which is of large size.

ON THE RELATIONS OF THE CONVOLUTIONS OF THE HUMAN CEREBRUM TO THE OUTER SURFACE OF THE SKULL AND HEAD. By PROFESSOR TURNER.

SOME years ago I planned in connection with an investigation into the arrangement of the convolutions of the human cerebrum, on which I was then engaged, a series of observations on the relations of the convolutions to the outer surface of the skull and of the scalp. Various circumstances have, however, combined to hinder me from carrying out this enquiry as fully as I should have desired. I have thought, however, that it might not be without interest to those engaged in the study of the anatomy and physiology of the human brain to record some of the leading facts, which I have up to this time ascertained, as a preliminary to a more extended memoir which I hope in due time to be in a position to publish.

In this communication, I shall direct attention to the general relations of the principal convolutions and sulci to the surface as I have observed them in the heads of adult men, and reserve to a subsequent memoir the relations of the corresponding structures in the female, and the variations in position, which to some extent occur in different individuals from variations in the configuration of the brain and skull.

In conducting an investigation of this kind, it is in the first instance necessary to have a clear conception of certain well-defined landmarks, which can be seen or felt when the outer surface of the skull and head are examined. The external occipital protuberance, the parietal and frontal eminences, and the external angular process of the frontal bone, are easily recognised structures, the position of which can be determined by manipulating the scalp, and still more readily on the surface of the skull itself. The coronal and lambdoidal sutures can also be felt through the scalp in most heads, and on the skull itself the position of the squamous, squamoso-sphenoid and parieto-sphenoid sutures, and the curved line of the temporal ridge can also without difficulty be determined.

With the aid of these structures, we may subdivide and map out each lateral half of the surface of the skull or scalp into ten well-defined regions or areas, and then localise within these areas the convolutions on the outer face of the hemisphere. The line of the coronal suture forms on each side the posterior boundary of the præ-coronal or frontal region. A vertical line drawn from the squamous suture upwards through the parietal eminence to the sagittal suture lies almost parallel to the coronal suture and subdivides the area of the parietal bone into a post-coronal or antero-parietal, and a præ-lambdoidal or post-parietal. The squamous part of the occipital bone between the lambdoidal suture and the occipital protuberance and superior curved line forms the post-lambdoidal or occipital region.

But these regions may be subdivided into smaller areas. The temporal ridge starting from the external frontal process curves backwards across the præ-coronal, post-coronal and præ-lambdoidal regions to the lateral angle of the occipital bone, and subdivides each of these regions into an upper and a lower area. The lower areas may be termed the inferior frontal or fronto-temporal, the lower antero-parietal, and the lower postero-parietal. The fronto-temporal area is bounded above by the temporal ridge, behind by the coronal suture, below by the fronto-sphenoidal suture. The lower antero-parietal region is bounded above by the temporal ridge, in front by the coronal suture, below by the squamous and sphenoido-parietal sutures, and behind by the vertical parietal line already referred to as subdividing the parietal region into an anterior and a posterior area. The lower postero-parietal region is bounded above and behind by the temporal ridge, in front by the above-named vertical line, and below by the posterior part of the squamous suture, and the parieto-mastoid suture. The upper area of the præ-coronal region consists of the frontal bone above the temporal ridge, and may be subdivided into a mid-frontal and a superior frontal by an antero-posterior line drawn backwards parallel to the frontal suture from the superior orbital border through the frontal eminence to the coronal suture. The upper areas of the post-coronal and præ-lambdoidal regions consist of the parietal bone above the temporal ridge, and are

bounded below by that ridge, above by the sagittal suture, and are separated from each other by the vertical parietal line already referred to. They may conveniently be named the upper antero-parietal, and upper postero-parietal areas. Eight areas may thus be defined on each side of the head situated beneath that portion of the scalp, which is so thin as to allow their extent and boundaries to be ascertained on external manipulation with a fair amount of precision.

That part of the side of the head, however, which lies below the line of the squamoso-parietal, sphenoido-parietal and sphenoido-frontal sutures, is so covered over by the thick mass of the temporal muscle that the determination of its limits on the head itself is attended with more difficulty. On the skull, however, it can be readily marked out, and may naturally be subdivided by the lines of sutures into the squamoso-temporal and the sphenoidal areas, which form the ninth and tenth areas on the side of the head.

Before I proceed to state the position of the convolutions within these different areas, it will be advisable to determine the regions in which the great fissures are situated, which subdivide the hemisphere into its five constituent lobes, frontal, parietal, occipital, temporo-sphenoidal and the Insula.

The Sylvian fissure commences immediately behind the posterior border of the lesser wing of the sphenoid bone, and in its course backwards and upwards, is covered by the great wing of the sphenoid where it articulates with the anterior inferior angle of the parietal. It then passes obliquely under cover of the anterior superior part of the squamous plate of the temporal, and appears in the lower part of the antero-parietal region, through which either it, or one of the small branches into which it not unfrequently divides, may be continued into the lower postero-parietal region.

The fissure of Rolando lies in the post-coronal region, through which it passes from below obliquely upwards and backwards, so that it traverses both its upper and lower subdivisions. The distance of this fissure from the coronal suture varies in different brains. I have seen its upper end as much as 2 inches behind the top of the suture, its lower end $1\frac{1}{2}$ inch behind the inferior part of the suture, but I have

also seen the upper and lower ends not more than $1\frac{1}{2}$ and 1·3 inch posterior to this suture.

The parieto-occipital fissure lies in the upper area of the præ-lambdoidal region close to its sagittal border. Its exact distance from the apex of the lambdoidal suture varies, partly from variations in the brain itself, and partly from the not unfrequent variations in the mode of ossification of the upper squamous part of the occipital bone. About 0·7 or 0·8 of an inch will express its average distance from the apex of that suture.

The relations of the parietal eminence to the surface of the hemisphere are apparently very constant. In the specimens I have examined, the hollow corresponding to the cerebral surface of this eminence was occupied by the supra-marginal convolution, which from this circumstance may appropriately be named the convolution of the parietal eminence.

As the fissure of Rolando, which I regard as the line of demarcation between the frontal and parietal lobes, extends obliquely upwards and backwards, in the post-coronal region, the great central convolutions which bound it in front and behind, and which we now name ascending frontal and ascending parietal, necessarily occupy a considerable share of its two subdivisions. To define their positions more exactly, I shall now state the contents of each subdivision.

In the lower antero-parietal area the lower third of the ascending frontal and parietal gyri are found, and from the anterior frontal arises, somewhat less than an inch behind the lower end of the coronal suture, the inferior frontal gyrus. Behind the ascending parietal, but separated from it by the intra-parietal sulcus, lies a small part of the supra-marginal gyrus or convolution of the parietal eminence. Below the ascending gyri extends a short segment of the Sylvian fissure, and quite at the lower and posterior boundary of the area a little bit of the superior temporo-sphenoidal gyrus appears above the squamous suture.

In the upper antero-parietal area the upper two-thirds of the ascending frontal and parietal gyri, which extend close up to the sagittal suture, are found. From the ascending frontal arise the superior and middle frontal gyri; the former arises 1·2 or 1·3 of an inch behind the coronal suture, the latter about

an inch behind the same line. Immediately behind the upper end of the ascending parietal gyrus, in the neighbourhood of the sagittal suture, a little bit of the postero-parietal lobule may or may not be seen; and below this a small portion of the convolution of the parietal eminence may appear.

The contents of the præ-lambdoidal region are as follows. In the lower postero-parietal area lies the hinder part of the convolution of the parietal eminence, behind which is the angular gyrus, and below this the posterior ends of the three convolutions of the temporo-sphenoidal lobe. In the upper postero-parietal area the postero-parietal lobule occupies the region close to the sagittal suture up to the parieto-occipital fissure; below it lies the upper portion of the angular gyrus, and a part of the convolution of the parietal eminence; in the more posterior part of the region the annectent gyri blend with the three convolutions of the occipital lobe.

In the post-lambdoidal region, which is comparatively small, the three convolutions of the occipital lobe succeed each other from above downwards.

The præ-coronal region is entirely occupied by the frontal lobe. In the inferior or temporo-frontal area is situated a large portion of the inferior frontal convolution, but a small bit of the middle gyrus may be seen at its posterior superior angle. The middle frontal area corresponds almost exactly to the middle frontal gyrus, and the superior frontal area to the superior gyrus. If the line of demarcation between the superior and mid-frontal areas were prolonged backwards for about an inch beyond the coronal suture into the upper antero-parietal area, the extent and position of the superior and middle frontal convolutions would be still more exactly defined. The frontal eminence may lie opposite the sulcus between the superior and middle frontal gyri, or if this sulcus, as is not unfrequent, is bridged across by a small tertiary convolution, this may correspond to the eminence.

The squamoso-temporal and the sphenoidal regions contain the anterior two-thirds of the convolutions of the temporo-sphenoidal lobe.

The lobes of the brain by no means precisely correspond to the areas of the cranial bones, after which four of them are

named. The frontal lobe is not only covered over by the frontal bone, but extends backwards for a considerable distance under cover of the parietal bone. If we accept, as I have elsewhere described[1], the fissure of Rolando as the posterior limit of this lobe, then the larger part of the post-coronal region corresponds with the frontal lobe, for not only does it contain the origins of the superior, middle and inferior frontal gyri, but also the ascending frontal convolution. The distance at which the fissure of Rolando lies behind the coronal suture was pointed out some years ago by M. Broca[2], was referred to by myself in my essay quoted above, and has subsequently been described by Prof. Bischoff[3], although the last-named writer, with Gratiolet and some other anatomists, regards the convolution which I have named ascending frontal, and not the fissure of Rolando, as the posterior limit of the frontal lobe. But even if this mode of looking at the ascending frontal gyrus were accepted, the frontal lobe would still not be wholly localized under cover of the frontal bone, for the superior, middle and inferior frontal gyri all arise behind the line of the coronal suture.

The occipital lobe also is not limited to the region covered by the squamous part of the occipital bone, but slightly overlapping the lambdoidal suture, extends forwards for a short distance into the back part of the upper postero-parietal area, and through the superior annectent gyrus reaches the parieto-occipital fissure.

The superior temporo-sphenoidal gyrus, though for the most part situated under cover of the squamous-temporal and great wing of the sphenoid, yet ascends into both the lower antero- and lower postero-parietal areas.

The area covered by the parietal bone, so far then from being conterminous with the parietal lobe of the cerebrum, is trenched on anteriorly, posteriorly and inferiorly by three of the other lobes of the brain. The convolutions of the parietal lobe are especially grouped around the parietal eminence, and in the interval between that structure and the sagittal suture.

The Insula or central lobe does not come to the surface, but

[1] *Edinburgh Medical Journal*, June, 1866, and separate publication "The Convolutions of the Human Cerebrum, topographically considered."
[2] *Sur le Siége de la Faculté du Langage articulé*, Paris, 1861.
[3] *Die Grosshirnwindungen des Menschen.* Munich, 1868.

lies deep in the Sylvian fissure, and is concealed by the convolutions which form the margin of that fissure anteriorly. It lies opposite the upper part of the great wing of the sphenoid and its line of articulation with the antero-inferior angle of the parietal and the squamous part of the temporal.

In conclusion I may again state that the above description is not intended to be exhaustive, but merely to afford a general conception of the chief relations of the convolutions to the surface, neither does it take into consideration the variations which may be occasioned by sex, race, or individual peculiarities. It may however help the physiologist and pathologist to map out on the surface of the living head, in a more precise manner than they have hitherto been able to do, the position of the convolutions, and in so far be of some use in the study of their functions and diseases.

TWO CASES OF PERSISTENT COMMUNICATION BETWEEN THE UMBILICAL AND PORTAL VEINS IN THE HUMAN SUBJECT. By J. A. Russell, M.A., M.B., *Demonstrator of Anatomy, University of Edinburgh.*

Mr Frank Champneys, in the sixth volume of this *Journal*, page 417, records a case of communication between the external iliac and portal veins through the umbilical vein, and gives references to previously recorded cases of communication between the portal vein and various veins of the abdominal parietes.

In connection with this subject, the occurrence in the dissecting-rooms of the University of Edinburgh last summer of two cases of persistent communication between the umbilical and portal veins where there was no obvious junction between the umbilical veins and any veins of the abdominal wall, may be worthy of record. In the first of those cases, that of a male, where there was a cirrhosed liver considerably enlarged by fatty degeneration, the canal of the umbilical vein, when traced towards the umbilicus, was found to end within the abdominal wall in a *cul de sac,* just opposite the navel. From this point to the suspensory ligament of the liver the vein was larger than a goose-quill, and thin walled. Just before joining the anterior edge of the falciform ligament it had a trilocular dilatation, also thin walled, of the size of a walnut, continuous with its wall and opening into it. An injection of melted lard failed to show any communication with parietal veins. Immediately upon passing from the dilatation and entering the suspensory ligament, which had much fat between its layers, the coats became greatly thickened and the vessel gradually enlarged, until, in the hepatic fissure for the umbilical vein, where it became continuous with the anterior division of the left branch of the portal vein, it was as big as the end of the fore-finger. Branches of varying size entered the liver substance on both sides of the fissure.

Proceeding from the dilatation and surrounding the vein were three pairs of thin-walled, highly tortuous vessels, as large as crow-quills at their origin. These vessels distributed branches to the fat in the falciform ligament, and in the umbilical and part of the portal fissures, and being diminished in size finally entered the liver substance in close contact with branches of the hepatic artery and portal vein. The ductus venosus was quite impervious.

In the other case, also a male with a very slightly enlarged liver, the connection between the portal and umbilical veins was exactly similar to the above, but the pervious channel of the umbilical vein did not reach the wall of the abdomen and only extended about a couple of inches from the liver, where it was suddenly obliterated within the falciform ligament, so that it formed, as it were, a diverticulum on the portal system.

In many of the cases to which Mr Champneys refers it is probable that the patency of the umbilical vein was caused by its fusion with Luschka's Vena parumbilicalis, and that this channel was afterwards enlarged by the obstruction due to liver disease. This could hardly be the case in the subjects noted above, as the umbilical vein was joined by no parietal vessels of any appreciable size, and such vessels could not fail to have been distended by a force sufficient to enlarge the umbilical vein, supposing the enlargement to have been due to liver-obstruction.

In connection with the communication between the portal and parietal veins, it may be interesting to note that Dr Monro's preparation, referred to by Mr Champneys, is still preserved in the Anatomical Museum of the University of Edinburgh (L 1 *Old Catalogue*, and 346 *New Catalogue*). The liver which Monro described as scirrhous is, however, more probably cirrhosed.

ON A PECULIAR DIGASTRIC MUSCLE—A VARIETY OF THE OCCIPITO-HYOID. By S. H. WEST, B.A., *Junior Student of Christ's Church, Oxford.*

THE muscle is of interest, as having been hitherto noticed, so far as I am aware, by only one anatomist, Mr Perrin, who has given a description of it in *this Journal*, v. 251.

I have now seen it in an aged male subject, symmetrically placed on both sides, digastric, slender, but of considerable length. On the left side it arose by a tendon, which expanded and blended with the fascia beneath the origin of the trapezius, below the superior curved line of the occipital bone, and was crossed, just beyond its origin, by the occipital nerve. From this origin it ran horizontally forwards about half an inch below the superior curved line, having swollen into a fleshy belly about one-fourth of an inch in diameter, as far as the sterno-mastoid muscle, upon the external surface of which it continued its course for about one-sixth of an inch. Here it suddenly bent downwards at an angle of about 120° to a point about one-fourth of an inch from the apex of the mastoid process, and three-fourths of an inch from the anterior border of the sterno-mastoid muscle, upon the external surface of which it lay throughout the whole of this part of its course. Here it became tendinous. Bending forwards to lie again parallel with its former course, i.e. horizontal, it became once more muscular, and was crossed by the anterior jugular vein. It then turned directly inwards over the anterior border of the sterno-mastoid muscle, lying behind the tendon of the digastricus. Here it received a special artery, which arose from the occipital just before the sterno-mastoid branch. Its fur-

ther course was downwards and inwards. It crossed the ascending pharyngeal artery (the occipital artery in this subject rising very high up), and passing immediately beneath the external and upon the internal carotid, came into relation with the stylo-pharyngeus muscle, lying parallel, but external to it. Continuing its course with this muscle, it passed between the superior and middle constrictors of the pharynx, and running under cover of the middle constrictor blended inferiorly with the inferior constrictor on its posterior aspect. Some of its fibres decussated with those of its fellow, and with those of the inferior constrictor of the opposite side. It was contained in all its superficial extent in a closely investing sheath of fascia, which bound it down to the parts beneath.

The muscle of the right side pursued a similar course, and differed only in the smaller size of its muscular bellies.

I was unfortunately unable to make out its nerve supply. It received its blood, as stated above, from the occipital artery by a special branch. The stylo-hyoid muscle was present on both sides. This muscle differs from those described by Mr Perrin, (i) in its curved course, (ii) in not being attached to the hyoid bone, and (iii) in receiving no accessory slips. Mr Perrin regards it as a differentiation of the stylo-hyoid of birds; and Professor Humphry, who refers to these peculiar abnormalities in his lectures (*Brit. Med. Journ.* June 21st, 1873), considers it as "an appendage, a superficial appendage, to the stylo-hyoid and digastric muscles," and explains its peculiar digastric condition as arising from causes similar to those to which he supposes the two bellies of the digastricus to be due. "The peculiarities of the digastric are in great measure due to the continued blending of parts of two of these layers" (the layers into which he states the primitive muscular sheet to be subsequently split), "the posterior belly being derived from the deep layer, the anterior belly from the superficial layer, and the intermediate tendon being a remnant of one of the transverse septa of the primitive unstratified muscle" (*loc. cit.*). For a more detailed account and for figures of this muscle, I may refer to Mr Perrin's paper quoted above.

EXPERIMENTAL RESEARCHES IN CEREBRAL PHYSIOLOGY AND PATHOLOGY[1]. By DAVID FERRIER, M.D., &c. *Professor of Forensic Medicine in King's College, London.*

(Abstract by the Author.)

THE paper contains the chief results of a research commenced with a view to test the accuracy of the views entertained by Dr Hughlings Jackson on the pathology of Epilepsy and Chorea. As is well known, Dr Jackson regards localised and unilateral epilepsies as dependent on irritating or discharging lesions of the convolutions about the corpus striatum.

In order to put this theory to the proof the author determined to expose the brain in various animals, and apply irritation to the surface. The method of irritation was suggested by the experiments of Fritsch and Hitzig, who had shown that contractions of definite groups of muscles could be caused in dogs by passing galvanic currents through certain portions of the anterior regions of the brain.

The progress of the research ultimately led to the endeavour to establish the localisation of cerebral function, not merely as regards motion, but also as regards sensation and the other faculties of mind.

The paper in the West Riding Reports gives the result of experiments on rabbits, cats, and dogs, and is confessedly only a preliminary instalment of a more extended research.

Since this paper was written the author has been engaged in the further prosecution of his inquiries, and at the British Association at Bradford, on September 19th, he gave an account of his more recent researches, at the same time entering more fully into the psychological explanation of many of the phenomena which are partly described in the published account. He has performed experiments on numerous monkeys as well as other animals, but reserves a complete account of his researches for the Royal Society.

The method of experimentation which the author has adopted is to place the animal under chloroform, and gradually expose the surface of the brain by successive trephinings and removal of the skull by means of the bone forceps. In this way he has been able to expose the whole hemisphere. After removal of the dura mater, the points of blunted electrodes in connection with Du Bois Reymond's induction coil are applied to the surface of the brain without injury to the cortical substance.

The first experiments recorded have special reference to the production of epileptic convulsions; and the mode in which the

[1] *West Riding Lunatic Asylum Medical Reports*, Vol. III. 1873. London, Smith, Elder and Co. 15, Waterloo Place.

attacks begin, and the march of the convulsive spasms, are accurately recorded.

It was found that in rabbits, cats, and dogs the application of the electrodes for a few seconds induced almost immediately, on some occasions after the lapse of a distinct interval, violent unilateral epileptic convulsions. When the electrodes were applied, one at the anterior, and the other at the posterior part of the hemisphere, the convulsions were complete and violent in the whole of the opposite side of the body. As a rule they commenced in the face, spread to the neck and upper extremity, and then invaded the hind-leg and tail. Dilatation of the pupil, clonic spasms of the jaws, foaming at the mouth, and loss of consciousness were induced when the fits were at their greatest intensity.

Occasionally the spasmodic convulsions remained localised in one or other limb, or in some one muscle or group of muscles, and frequently, instead of a hasty epileptic attack, a series of choreic twitches alone were manifested, without any affection of sensibility or consciousness.

The march of the spasms is shown to be quite in accordance with the clinical observations of Dr Hughlings Jackson in cases of unilateral epilepsy in man. Peculiar variations in the mode in which the attacks commenced, depending apparently on the position of the electrodes on the surface of the brain, led the author to approximate the electrodes and to apply very limited irritation, in order to discover whether the convulsive spasms were not due to over violent irritation of localised centres in the brain, whose special function is to govern and direct the action of these muscles for definite purposes, possibly such as might indicate volition and intelligence.

The results were such as to indicate, with a beautiful degree of exactitude, the localisation in certain definite and easily defined regions the cerebral centres for various apparently purposive combined movements of the muscles of the limbs, as well as of the tail, the facial muscles, and the muscles of the jaws and tongue. These are all situated in the anterior parts of the brain in advance of the Fissure of Sylvius, and the individual centres are marked off in the various external convolutions, of which woodcuts are given. The general plan is, that in the superior external convolution anterior and posterior to the crucial sulcus the various movements of the paws, legs, and tail are centralised, and it is shown that the differentiation of these centres is to a great extent characteristic of the animal's habits; the centre from the fore-paw in cats being much more highly differentiated than in dogs and rabbits.

The middle external convolution governs movements of the eyelids, face, and eyes, while the inferior and the Sylvian govern various movements of the whiskers, angle of the mouth, depressors of the lower jaw, and tongue.

In the convolutions posterior to the Fissure of Sylvius certain movements are described as resulting from irritation, viz. of the ears, eyes, &c. In the paper as yet published no attempt is made to explain the signification of these; but the author, from his later

experiments, indicated at the Meeting of the British Association that he had been able to obtain indications of the situation in these regions of the centres of special sense, sight, hearing, and smell. These results and conclusions are however not as yet detailed fully. The author indicates, in a note in the paper published in the West Riding Reports, that he had at that time explored the brain of the monkey, and satisfactorily localised the regions and the homologues of the centres already discovered in the brain of the cat, rabbit, and dog.

One of the more important conclusions drawn from the experiments is, that the region which governs the movements of the mouth and tongue in cats and dogs is the homologue of what is known as Broca's convolution in man, viz. the posterior part of the inferior frontal.

This, it may be stated, is further borne out by experiments on monkeys.

The pathology of Aphasia is thus rendered comparatively simple. The memory of words is situated in that part of the brain which governs the movements of articulation. It is shown, however, by the experiments, that the brain is symmetrical, and that the corresponding part of each hemisphere produces exactly the same effects on opposite sides of the body. Generally the action is unilateral and crossed, but as regards the mouth the action is almost bilateral, and hence disease of one or other side alone does not cause paralysis of the articulating muscles, because the other side is able to govern as before. The occurrence of loss of speech with lesion of the left side is attributed to the fact that most people are left-brained, and that therefore a lesion of the left side causes such an interference with the voluntary recalling of words that the person is speechless, not because memory of words is utterly lost, as this exists in the undamaged side, but because he is unable to lay hold of the word which he wishes to express. With the education of the other side however the individual recovers the power of speech. During the interval of recovery of speech only automatic expressions or interjections are uttered, which are evoked by a sort of reflex action, and unconnected with volition. Among other points discussed is the hypothesis advanced by Dr Broadbent, that associated movements of the body are bilaterally coordinated in each hemisphere. The author thinks the experiments which he gives indicate not an *anatomical* but a *physiological* coordination through the media of the lower ganglia.

The results of experiments in the corpora striata, optic thalami, corpora quadrigemina, and cerebellum, are also detailed.

The corpora striata are shown to be motor in function, and to govern all the muscles of the opposite side; representing all the muscles directed by the hemispheres, and having a physiological subordination to these as higher centres.

The optic thalami are shown to have no motor function, and the author attributes the interference with motion, which is sometimes described in connection with disease of these ganglia, to affection of the motor strands with which they are in close relation.

The corpora quadrigemina have a special relation to the eyes and also to the extensor muscles.

Irritation of the nates causes great dilatation of the pupils. The action is crossed, but powerful irritation easily acts on both sides of the body. Trismus and opisthotonus are induced when these ganglia are powerfully stimulated.

The cerebellum is shown to have a function which has never been allotted to it, viz. to be a coordinating centre for the muscles of the eyeballs. The author has only given the results of his experiments on the cerebellum of rabbits, but he has since extended and confirmed them in cats, dogs, and monkeys.

The various lobules of the rabbit's cerebellum are shown to have the power of directing the eyes in certain definite directions.

These cerebellar oculo-motorial centres are brought into relation with the cerebellum as a coordinating centre for the muscles concerned in the maintenance of the equilibrium, and these functions are indicated as mutually depending on each other.

A more complete exposition of the facts of experiment, and an account of the results obtained from a further investigation of the brains of the various classes of the vertebrata, is in process of publication.

TWO INSTANCES OF IRREGULAR OPHTHALMIC AND MIDDLE MENINGEAL ARTERIES. By JOHN CURNOW, M.D. Lond., *Professor of Anatomy in King's College, London.*

I. IN the summer session of 1872 I dissected an interesting irregularity affecting the right middle meningeal artery. The foramen spinosum was much smaller than usual, and through it passed a very slender artery from the internal maxillary, which after giving off a few small twigs to the Gasserian ganglion, entered the hiatus Fallopii. The foramen ovale transmitted the inferior maxillary nerve only. A large artery arose from the trunk of the ophthalmic soon after it had entered the orbit, and passing out through the sphenoidal fissure divided into two branches which were distributed as the normal branches of the middle meningeal, except that they ran backwards above the inferior border of the parietal bone, which was consequently smooth, and without its usual deep grooves. In addition, a large artery was given off from the internal carotid in the cranium, and coursed backwards over the basilar processes of the sphenoid and occipital bones almost to the foramen magnum. This recurrent branch was also present on the left side, but the left middle meningeal and ophthalmic arteries were quite regular.

A less complete instance than this is depicted by Barkow[1] (Tab. XVII. figs. 1 and 2), in which the anterior portion of the middle meningeal comes from the ophthalmic through the sphenoidal fissure into the cranium; but the posterior branch is given off from the internal maxillary and enters through the foramen spinosum.

II. During the past winter session I met with an arrangement almost the exact reverse of the preceding.

Besides its ordinary branches, the left middle meningeal gave off a large artery, which entered the orbit through the sphenoidal fissure, and from which all the regular branches of the ophthalmic arose, with the single exception of the arteria centralis retinæ. It ended in a long dorsal artery on the nose, running downwards as far as the tip, and supplying angular and lateral nasal offsets. The ophthalmic from the internal carotid was a very small twig; it passed through the optic foramen, gave off the arteria centralis retinæ, and terminated by joining the posterior ethmoidal branch of the irregular artery from the middle meningeal. The trunk of the left facial was smaller than usual and ended as the superior coronary, from which proceeded the septal branch as usual. On the right side, the ophthalmic, middle meningeal, and facial arteries were quite regular.

Anastomotic twigs between the middle meningeal artery and the lachrymal branch of the ophthalmic are always met with; and Cruveilhier mentions the occasional origin of the lachrymal from the middle meningeal. Barkow figures an example of this irregularity, and I have also seen it.

These anomalies are obviously due to the suppression of the main trunk, and compensatory enlargement of the anastomosing branch, similar to the mode of formation of the abnormal obturator from the epigastric, the dorsal artery of the foot from the anterior peroneal, &c. I have failed to find any reference to a deviation from the regular origin so extensive in this direction as those which I have recorded above. The only irregular origin of the ophthalmic described by Quain (Pl. XIII. fig. 8) is of quite a different type. In this the place of an absent internal carotid artery is supplied by two branches of the internal maxillary, which enter the cranium through the foramen rotundum and foramen ovale respectively, and from their junction the ophthalmic is given off.

[1] *Comparative Morphologie des Menschen und der menschenähnlichen Thiere.* 5ter Theil.

NOTE BY EDITORS.—Blandin (*Anatomie Topographique*, p. 147) mentions an accessory ophthalmic artery arising from the middle meningeal, which sometimes also gives origin to the lachrymal artery. C. Krause states (*Handbuch der mensch. Anat.* p. 892) that he once saw the ophthalmic artery arise from the middle meningeal and pass through the sphenoidal fissure. Tiedemann and Dubrueil have also recorded similar cases.

REVIEWS AND NOTICES OF BOOKS.

The Anatomist's Vade Mecum. By ERASMUS WILSON, F.R.S. Edited by G. BUCHANAN, M.D. 9th edition, London, 1873.

THE new edition of this well-known *Student's Manual* has been prepared by Dr George Buchanan with the assistance of Mr Henry E. Clark. To bring it up to the present state of knowledge, many additions have been made both to the text and wood-cuts.

Handbuch der systematischen Anatomie des Menschen. Von Dr J. HENLE. 3rd vol. 2nd division, 2nd part. Brunswick, 1873.

PROFESSOR HENLE has now completed his great work on Human Anatomy, by the publication of the descriptive anatomy of the cerebral and spinal nerves and the sympathetic system. Like all the previous parts, it is very fully illustrated by original drawings from dissections. Not only are the more usual arrangements of the nerves described, but reference is made to the principal variations which have been recorded by other anatomists. The work constitutes the most complete treatise on human anatomy in any language. It has lost somewhat in unity by its mode of publication, as do all scientific treatises published in parts, with long intervals between the appearance of the different parts; for the earlier volumes are necessarily not so well posted up to the present state of the subject as the one just finished. So great, however, has been the demand for the earlier numbers, that the one on the bones has reached a third edition, and second editions of those on the ligaments, muscles and viscera have been issued. We may congratulate Professor Henle on having lived to complete so enduring a memorial of his learning, industry, and power of description.

The Comparative Anatomy of the Domesticated Animals. By Prof. CHAUVEAU. Translated and edited by GEORGE FLEMING, *Vet. Surg. Royal Engineers.*

THE reviewer regrets that in the notice of this work in the last number of the *Journal*, in consequence of an oversight, it was intimated that there is an omission of the mention of the sources from which the drawings, which are not original, are derived. This, however, is fully done in a table of illustrations which immediately follows the table of contents.

The Harveian Oration, 1873. By GEORGE ROLLESTON, M.D., F.R.S., *Linacre Professor of Anatomy and Physiology, and Fellow of Merton-College in the University of Oxford.* London, 1873.

IN this very able and characteristic oration, full of humour and quotation, and full likewise of evidence of assiduity and acuteness of perception, Professor Rolleston first expounds certain advances recently made in the anatomy and physiology of the circulatory organs, and gives an account of the 'moderator band' found by him in the heart of a cassowary, pointing out the homologous band to be not unfrequently seen in the human heart. Secondly, he gives "the as yet unrecorded history of one of the many attempts to rob Harvey of his rightful rank in the noble army of discoverers, which were made in the latter half of the seventeenth century." It is the claims which have been put forward on behalf of Walter Warner, the editor in 1631 of *Harriott's Algebra*, to the discovery of the circulation of the blood. Having by a circuitous route come upon Walter Warner's original MS., the Professor gives some quotations from it which point out "the real merits of the claimant before us," and show that Harvey might have truly said

"We were the first that ever burst
Into that silent sea."

The oration concludes with a glowing eulogium of the scientific character, mental culture and style of Harvey, based upon a perusal and re-perusal of his works.

The Convolutions of the Human Brain. By DR ALEXANDER ECKER. Translated by JOHN C. GALTON, M.A. London, 1873.

IN our Half-yearly Report on the Progress of Anatomy, IV. p. 157, we directed attention to Prof. Ecker's Monograph on tho Cerebral Convolutions, published in that year, and we briefly criticized some of the statements contained in it. A translation of this Monograph by Mr Galton has now appeared, and will be found useful to those members of the medical profession, happily an increasing number, who are studying the convolutions of the brain, with the object of obtaining a more exact knowledge of cerebral physiology and pathology. Mr Galton has done good service in compiling a more complete bibliography than the author had himself appended to his Monograph.

REPORT ON THE PROGRESS OF ANATOMY.

By PROFESSOR TURNER[1].

OSSEOUS SYSTEM.—Wenzel Gruber has prepared a Monograph on the OS ZYGOMATICUM BIPARTITUM in men and mammals (*pamphlet*, Vienna, 1873). He gives in the first instance an account of the cases observed by other anatomists, and then proceeds to describe ten cases of subdivision of the malar bone in the human subject, which have come under his own observation, nine of which were in male crania, and one in a female. In five specimens the subdivision was on both sides, in two on the right, in three on the left. The malar bone was subdivided into a superior or orbital, and an inferior or temporo-maxillary part; the latter of which was always smaller than the former, and sometimes so small as to be little more than the inferior border of the bone. In all the cases the zygomatic process of the temporal articulated with both the orbital and temporo-maxillary portions. Gruber then describes an example of bipartite malar bone in *Phascolomys wombatus*, and the arrangement of the malar in various mammals.——Gruber also describes VARIATIONS IN THE FORAMEN MENTALE (*Reichert u. du Bois Reymond's Archiv*, 1872, 738), and to the same *Archiv*, 1873, p. 195, *e. s.*, he contributes a number of other VARIATIONS IN THE ANATOMY OF THE CRANIAL BONES, viz. a special ossification at the junction of the malar and superior maxillary bones; an unusually deep depression of the incisive fossa of the upper jaw; an adult male skull in which the pre-maxillary element of the left upper jaw was marked off by a fissure which extended from the posterior internal end of the palatine foramen to the posterior internal angle of the lacuna incisiva; a skull showing accessory processes of the upper jaw prolonged backwards to make good the posterior border of the hard palate, which, on account of imperfect development of the hard palate, would otherwise have been deficient; two additional ossifications in the hard palate of a young male skull. On p. 208, a supplementary memoir to that on the *os zygomaticum bipartitum*, already referred to.——In the same *Archiv*, p. 649, H. V. Jhering gives an account of the DEVELOPMENT OF THE FRONTAL BONE. He states that this bone possesses, in addition to the two chief centres of ossification, six accessory pieces; the two least important of which are for the nasal spine and for the inner wall of the orbit in the region of the fossa trochlearis, the two more important on the external lateral angles of the bone. The last are for a time independent bones, but as a rule are blended with the frontal at the time of birth.——J. Balandin discusses the question of the CURVATURES OF THE HUMAN SPINE (*Virchow's Archiv*, LVII. 481). After an historical

[1] To assist in making the Report complete Professor Turner will be glad to receive separate copies of original memoirs and other contributions to Anatomy.

introduction, he details his own observations and experiments, and concludes as follows: The thoracic curve has been observed by him at the 2nd month of embryo-life. The first indication of consolidation appears in the 4th month. He believes that it is conditioned in the first instance by the laying down of the skeleton, and that it is perfected by the pressure of the growing and expanding thoracic viscera. The cervical curve commences in the 3rd month of extra-uterine life, and is consolidated in the 4th to the 5th month; that is at the time when the infant sitting in the arms of its nurse raises its head and removes the chin from the thorax. The lumbar curve appears at the end of the first, or the beginning of the 2nd year. It is not usually consolidated until growth is completed. It takes place, therefore, at the time when the child assumes the erect position by the extension of the lower limbs on the trunk. Both the cervical and lumbar curves are occasioned by the action of muscles on the spine.

Muscular System. — P. Lesshaft investigates (*Reichert u. du Bois Reymond's Archiv*, 1873, No. 1) the arrangement of some of the Muscles and Fasciæ in the region of the Urethra. He has dissected the perineum in 210 subjects, and eighty specimens of perineal organs after removal. His conclusions are as follows: The *M. constrictor urethræ membranaceæ seu Wilsoni* encircles the pars membranosa, arises from the walls of the venous plexus of Santorini, lies on both sides of the urethra, and ends in the superior process of the septum perineale. The upper part of the muscle reaches the sides of the prostate. It acts as a constrictor of the urethra, and, from its attachment to the walls of the veins, as a relaxator penis. It is separated from the inner layer of the levator ani by a definite membrane. Three *Mm. transversi perinei* are situated between the inner wall of the pelvis and the perineal septum; the *superficialis* occurs only as a rare anomaly in 7·74 per cent. of the subjects examined; the *medius* is wanting only in exceptional cases, it is the superficial transverse muscle of authors; the *profundus* is the most constant. They tighten the perineal fascia, and the *profundus* acts indeed as a dilatator urethræ and compressor of Cowper's glands. The *M. transversus urethræ* is placed in front of the urethra; it arises from the inner surface of the descending pubic ramus, and ends in front of the middle of the urethra: one fasciculus commonly passes above the vena dorsalis penis to the fascia penis. It acts as a dilatator urethræ, and contributes, by compressing the dorsal vein, and by tightening the fascia penis, to erection. The *caput accessorium M. bulbo-cavernosus* is an anomalous head of the *M. bulbo-cavernosus*, which arises from the tuber or ramus ischii: it lies in the same layer as the *M. transv. perinei medius*. The pelvic fascia stretched between the pelvic walls, the rectum and the bladder, gives off on each side two lateral descending processes, an internal and an external, and a median process. The internal obturator muscle lies between the side wall of the pelvis and the external descending process. The ano-perineal fascia is a prolongation of the fascia

glutea: it subdivides behind the border of the m. transversus medius into a superficial lamina, which passes forwards into the fascia penis, and a lamina profunda, which ends in the pubic arch. The pelvic fascia, the anterior part of its internal descending process, its median process, the lamina profunda of the perineal fascia, and the lower inner portion of the synchondrosis pubis, form a *capsula prostato-urethralis*, in which the prostate, the membranous part of the urethra, Cowper's glands, the colliculus bulbi intermedius, the *Mm. constrictores*, the Mm. transversi perinei profundi, and transversi urethræ, the internal pudendal nerves and vessels, the prostatico-urethral, bulbous and bulbo-urethral veins, are enclosed.——In the same *Archiv*, p. 126, A. v. Brunn describes a variety of the *M. interosseous dorsal. manus* II, which possesses a third head arising from the dorsum of the unciform bone.——Henry S. Williams compares (*Trans. Connecticut Acad. of Arts, and Sc.* II., Part 2 1873) the MUSCLES OF THE HUMAN AND CHELONIAN SHOULDER-GIRDLES. His object has been to show the importance of the positions and relations to each other and to the axes of the bones of the areas of origin and insertion of muscles. The teres major and scapular part of the latissimus in man he regards as homologous with the chelonian teres major; the teres minor with the m. scapulo-acromio-humeralis; the deltoid with a muscle arising from near the scapulo-acromial angle to near the medial extremity of the acromion; the supra-spinatus with the M. acromio-humeralis secundus, and M. coraco-humeralis secundus; the coraco-brachialis with the M. coraco-humeralis primus; the biceps with the M. coraco-ulnaris and M. coraco-radialis; the subscapularis with the MM. scapulo-humeralis secundus and tertius: the long head of the triceps with the chelonian muscle which arises from the rim of the glenoid cavity on the outer side, and which, after joining a bundle which possesses a humeral origin, is inserted into the dorsal side of the proximal head of the ulna.——An abstract of a Memoir on the MECHANICAL CONDITIONS OF THE RESPIRATORY MOVEMENTS IN MAN, by A. Ransome, is in *Proc. Roy. Soc. Lond.*, Nov. 21, 1872.——G. R. Wagener concludes, from his observations on the STRUCTURE OF TRANSVERSELY STRIPED MUSCLE (*Schultze's Archiv*, IX. 712), that the fibrilla is the ultimate constituent of the fibre. That all the forms of transverse discs take their origin only out of the subdivision of the contractile substance in different parts of the fibrilla, and that the intermediate discs (*Zwischenscheiben*) are not definite structures.——An elaborate memoir by Rudolf Arndt, on the ENDING OF NERVES IN TRANSVERSELY STRIPED MUSCLE-FIBRES, occupies upwards of 100 pages of *Schultze's Archiv*, IX. 481, and is illustrated by three large plates. He believes that he can distinguish between the mode of termination of the motor and sensitive nerves in a muscle. The chief difference between them being this: the motor-nerves are broad, medullated fibres which terminate within the sarcolemma, and are therefore intra-muscular, whilst the sensory fibres are narrow fibres, poor in medulla, and in part pale and non-medullated, which end outside the sarcolemma either in sensitive 'plaques,' or in excessively delicate penicillated

filaments. These are extra-muscular, and not unfrequently the sensitive nerves encircle the muscular fibre.

. CONNECTIVE TISSUE.—Arnold Spina relates *Observations on the structure of Tendons* (*Med. Jahrb.* III. Heft 1873). After referring to the more recent researches on the subject, he describes his method, and then proceeds to give an account of his own observations, which may be summarized as follows:—The development of tendon may be considered in three stages. First Stage. Tendon consists of three fibrillary fascicles, between which run rows of solid cells. The cells are embraced by a substance that does not swell with acids, and refracts light strongly. When teased it appears in the form of thorn-shaped processes. A transverse section shows it in the form of a ring, which surrounds the cell and sends out membrane-like processes which separate the connective-tissue fascicles from each other. Longitudinal sections show this substance as a ring which surrounds the ellipsoid cells. Second stage. The connective-tissue fascicles have become thicker, and the rows of cells have become longer. The cells have become flattened to a domino shape, and towards the ends of the row they are seen to be gradually narrower, so that the row is pointed in shape at its extremities. The substance enclosing the cells has acquired a ladder-shape, the sides of which, at the extremities of the cell-row, are continued in a single line. But, besides, threads of the same substance are seen stretching along the middle line of the cell-row. These cannot be considered as being anything else than the thickened cell-wall. These terminal and median threads are to be identified as elastic fibres. The rows of cells are no longer perfectly parallel, but come into contact with each other; the threads of one cell-row often becoming continuous with those of another. The sheaths of the connective-tissue bundles, visible only in the transverse sections, appear in this stage also as membranous processes of the cell-wall. Third stage. The rows of cells have changed to flat bands, some of which are striated longitudinally and transversely; others are non-striated. The striated appearance is produced by elastic matter which persists in the above-mentioned ladder-form. These threads can be traced a long way into the fibrillary tissue, and can be diagnosed as elastic fibres. They form a wide-meshed net which penetrates the connective tissue, the meshes being the cause of the known bulgings of the tendon fascicles. *En résumé.*—(1) The elastic tissue of tendon takes its origin on the surface of the cells. (2) The fine sheaths in tendon are formed by membrane-like processes of the cell-wall. (3) The cell-rows become striated and non-striated bands.

EPITHELIUM.—P. Langerhans communicates (*Virchow's Archiv*, LVIII. 83) some observations on *stratified epithelium*, more especially with reference to the distribution of epithelial cells possessing denticulated processes. He figures various examples from the cornea, rete Malpighii, œsophageal and vesical epithelium.

PLACENTA.—F. Winkler communicates observations on the *Human Placenta* (*Archiv für Gynaekologie*, IV. 238). He describes his methods

of injecting and hardening the organ. He considers that the fœtal part of the placenta is of no architektonic importance, and applies the term *Netto-placenta* to the ideal organ as it would appear after the elimination of the entire fœtal portion, whilst to the complete organ he gives the name *Brutto-placenta.* The *Netto-placenta* has a cavernous structure, bounded on the uterine aspect by the *Basalplatte*, on the fœtal aspect by the *Schlussplatte*, between which the wide-meshed cavernous trabecular structure is situated. The uterine *basalplatte* or serotina consists of two layers, an external small-celled and an internal large-celled. The small-celled layer consists of a preponderating portion of intercellular substance, mostly homogeneous, but here and there streaked. It is through this layer that the separation of the mature placenta takes place. The large-celled layer contains the well-known colossal decidual cells imbedded in a sparse intercellular tissue. The intercellular substance increases in importance on the fœtal aspect of this layer, and the trabeculæ, which are formed of homogeneous intercellular substance, with small, round, spindle, rarely star-shaped, connective-tissue corpuscles scattered in it, are continuous with and derived from this layer. Except in the cotyledonary septa, in which for short distances the large cells penetrate, the whole of the rest of the *Netto-placenta* consists of this kind of connective tissue. The *Schlussplatte* is a subchorionic layer, and bounds the blood-caverns on the fœtal aspect of the placenta; like the inner layer of the *Basalplatte*, it consists of homogeneous intercellular substance with cells scattered in it. Vertical trabeculæ extend through the placenta from the *basal* to the *schlussplatte*, from these vertical bands transverse or oblique trabecles proceed, to trace which outwards is almost impossible on account of their delicacy. The *basalplatte*, the *schlussplatte*, and the trabecles, bound the maternal blood-spaces, which are everywhere, except where large villi have broken through the wall, lined by an endothelium. The placenta possesses in its *basalplatte*, especially at the places of intersection of the cotyledonary septa, apertures through which the cavernous spaces within the placenta communicate with the venous sinuses of the uterus, which apertures open either directly or by a sort of vascular tube three or four *Mm.* long into the blood-caverns. Winkler believes that the histological detail supports the opinion of Virchow, that all the vascular spaces of the *Netto-placenta* consist of ectatic capillaries with consecutive cavernous formations. He believes that muscle-elements exist in the layer of colossal cells of the serotina. He distinguishes three kinds of chorionic villi: *a.* those which become obliterated and end in the *schlussplatte*, without ever penetrating into the blood-caverns: *b.* short vascular villi which end directly in connection with the blood-caverns lying immediately under the *schlussplatte;* only that portion of these villi which hangs free in the blood-caverns possesses a recognizable epithelial envelope: *c.* long and many-branched vascular villi which penetrate deeply into the placenta, some reaching as far as the *basalplatte.* These villi are either imbedded in the maternal tissue of the placental trabeculæ, in which case they are without epithelium; or they lie free in the cavernous spaces, and then possess the well-

known epithelial investment. In their whole extent the stems of the villi are intimately connected with the placental trabeculæ. He believes that a system of juice-canals (*Saftcanälchen*) permeates the connective tissue of the chorion, the gelatine of Wharton, and even the amnion, the epithelial layer of which possesses apertures like the serous investment of the diaphragm. He finds these juice-canals to be connected with the blood-vessels, as Carter (see this *Journal*, IV. p. 97) had experimentally shown in other tissues. Winkler opposes the statement of Reitz that the blood-vessels of the villi lie in perivascular spaces. From the examination of a nine-weeks old abortion he describes the maternal part as consisting of a small-celled tissue in which many spaces exist, lined by a single layer of epithelium, which is often cylindriform in shape. These spaces he regards as utricular glands, and into them villi, presenting the usual epithelial investment, project, though without filling the space. Other villi projected simply into the maternal tissue without being contained in a space lined by epithelium; he considers that these villi have burst through the walls of the glands in the process of development and growth of their various branches. By a continuance of this process of growth they then press against the walls of the maternal blood-spaces, and parting asunder the endothelial cells, project into the blood-caverns. As the villi in the mature placenta which are invested by the maternal tissues are naked, the pressure of growth must have made to disappear both the epithelium proper to the villus, and that of the utricular glands. He entirely disagrees with Friedländer's view, that utricular glands exist in the placental area, and has never seen a trace of a gland either in the serotina or placental trabeculæ, neither has he seen the double layer of cells on the villi which Jassinsky (*Report* III. p. 203) has described.——C. Hennig publishes researches on the *Human Placenta* (*pamphlet*, Leipzig, 1872). He makes a careful examination of a uterus in the third month of gestation, in which he found no sharp line of demarcation between the placental and non-placental part of the fœtal membranes. The uterine mucous membrane has considerable thickness, and in addition to a cavernous venous reticulation possesses glands both in the decidua vera and serotina. They disappear from the reflexa in the earlier months, and in the later months they are no longer recognisable with certainty beyond the placental area, the only indications of their existence being the cribriform apertures of the decidua; their ciliated epithelium is much more difficult to be recognised than on the free surface of the mucous membrane, and appears to be converted both in the serotina and vera into tessellated scaly epithelium. The chorionic villi consist of a gelatinous connective tissue containing corpuscles, of the fœtal vessels, and of a fœtal epithelium consisting of a nucleated pavement epithelium, which sometimes peels off like a glove from a finger: this epithelium is the internal cells of Goodsir, the inner epithelium of van der Kolk. Further, in many localities, though he admits not universally, one or two other coverings are found: a maternal epithelium of serotina cells, the outer cells of Goodsir and van der Kolk, the gland-organ of Ercolani; and the wall of the

maternal vessel—the external membrane of Goodsir and van der Kolk. The villi of the chorion grow through the reflexa and vera, and penetrate in the later months of pregnancy into the serotina. At the end of pregnancy the serotina forms the thin layer which covers the placental spot and the covering of the uterine aspect of the placenta, from which latter proceed the processes which penetrate between the cotyledons. Although on the inner surface of a uterus from which the placenta has just been separated the blind ends of young utricular glands are situated, the tubes of these glands cannot be seen in the placental part of the serotina. For not only the glands themselves, but the interglandular tissue, are converted in the latter half of pregnancy into a framework of delicate columns and tubular spaces between, and in which epithelial-like and often nucleated colossal cells are situated. These columns and tubular spaces arise partly from the basis tissue (*Grundgewebe*) of the placenta, indeed from the connective tissue of the serotina and partly from new-formed glands, which cannot however be recognised as such, as they are imbedded in the growing cells, and their epithelium is completely modified. The serotina cells arise partly from the endothelium of the young glands, partly from the connective-tissue corpuscle of the intermediate tissue, and probably from cells which have migrated out of the blood-vessels. The largest of these cells have several nuclei, as many as nine have been seen, which nuclei are disposed like a cylindrical epithelium: other modifications of these cells are also described. Hennig concludes his account of the structure of the placenta with the following statement: "The chorionic villi reach in the second and third months the decidua reflexa, through which they grow, then they penetrate the vera, as Spiegelberg had already shown, and in the last months pass through the most inferior parts of the serotina, not only into the mouths of the glands, but into spaces in the intermediate tissue: finally, they penetrate the blood-vessels as when they enter the marginal sinus of the placenta."——In consequence of the observations of W. Turner on the placenta of *Orca gladiator* (Abstract in *Report*, v. 383 and vi. 439), and of Winkler and Hennig on the human placenta (Abstracts as above), G. B. Ercolani has been led to re-investigate the *structure of the Placenta in the Mammalia* (*Mem. dell' Accad. d. Scienze di Bologna.* T. iii. 1873), more especially with reference to the determination of the part which the utricular glands take in the formation of the maternal part of the placenta and in the nutrition of the fœtus. In his previous very elaborate memoirs (Abstract in *Report*, vi. 439), Ercolani had maintained that the utricular glands furnish materials for the nutrition of the embryo only in the early period of its development, and that the villi do not penetrate into their mouths; but that subsequently a new maternal glandular organ is formed, consisting of secreting follicles, lined by a pavement epithelium, into which the villi of the chorion fit, and from the secretion formed by which the fœtus is nourished. In his new memoir he describes additional researches on the sow, and summarises the differences between the non-gravid and gravid uterus of that animal as follows:—1st. A want in the

gravid of the well-marked rugæ of mucous membrane with numerous folds such as are seen in the non-gravid. 2nd. A remarkable increase in the gravid of the muscular layer and of the blood-vessels, especially those of the mucosa. 3rd. A diminution in thickness in the gravid of the glandular layer. 4th. A change in the internal superficies of the mucosa, which is smooth in the non-gravid, uniformly subdivided into fine ridges and furrows in the gravid uterus. It is into these furrows, which constitute the new-formed glandular organ, that the villi of the chorion are lodged. 5th. The utricular glands are more developed in the gravid, which he attributes to being more distended with contents, and to an increased thickness of the epithelium. In the non-gravid the cylindrical epithelium of the glands is twice the thickness of that covering the free surface of the mucosa. In the gravid uterus the epithelium covering the free surface of the mucosa is pavement-like when seen on its free surface. The glandular organ in the sow is not so completely subdivided into distinct crypts as was described by Turner in *Orca*, for the dissepiments or ridges between the crypt-like furrows are less perfectly developed. Scattered over the surface of the gravid mucosa are circular areas circumscribed by a raised border formed by the rounded ends of the minute ridges of the mucosa: in the centre of each of these areas the mouth of an utricular gland opens; the cylindrical epithelium of the gland contrasts with the pavement-like epithelial covering of the area itself. He considers that the mouths of these glands are plugged up during the early period of pregnancy by rounded growths such as Eschricht had described, which are not villi, from the surface of that part of the chorion which lies opposite the mouths of the glands; whilst in the later periods the hyperplasy of the connective tissue of the mucosa around the mouths of the glands completely obliterates their apertures. Hence he concludes that natural obstacles exist to the flow of the secretion of these glands out of their mouths, so that it cannot be absorbed by the villi, and does not take a part in the nutrition of the fœtus. He admits however that in the mare and ass the utricular glands do secrete freely during the whole period of pregnancy. He meets the objection raised by Turner to the secreting function of the follicles or crypts of the new-formed organ, because they are lined by a pavement epithelium, by the statement that the objection is "eliminated by the fact of the constant and complete change in the physical and anatomical qualities of the epithelium in the uterus of the sow, which, from being cylindrical as it is in the unimpregnated uterus, becomes pavement-like in pregnancy," and by the further statement, that there are other instances in the vertebrata of secreting organs with pavement epithelium, as the sebaceous, sudoriparous and ceruminous glands. As an additional illustration that the utricular glands are not concerned in placental formation Ercolani adduces the structure and mode of growth of the cotyledons in the sheep, in which in the non-gravid state the position of the future cotyledons is mapped out on the mucosa by areas in which the utricular glands are absent, though they exist in abundance at the borders of these areas. In the changes which take place during pregnancy a new formed glandular

organ takes place in the cotyledon by an abundant cell-production, producing follicles into which the villi of the chorion fit. He states that his previous observations on the production of a new-formed maternal cellular organ in the early and later stages have been confirmed by Romiti in the intermediate stages. In opposition to Leydig, who holds that utricular glands do not exist in the Muridæ, he states that he has seen them, though few in number, in the uterine mucosa of *Mus musculus*. They are simple, slightly sinuous, not very long, and have a cylindrical epithelium. The epithelium is shed from the areas to which the ova are attached, and a rich new formation of cells takes place, by which the ova are embraced: this formation is not derived from the utricular glands, because in these localities they lose their internal epithelium during pregnancy, are augmented in volume, and show in their interior an amorphous transparent substance. In the non-placental parts of the mucosa the glands undergo no change. The Hydroperion in the human female possesses a nutritive function, and is secreted by the utricular glands in the earliest period of gestation. The disappearance of the hydroperion, and the impossibility of demonstrating utricular glands in the later period, he considers as correlated conditions. He believes, in opposition to Hennig, that the utricular glands in the human female take no part in the formation of the placenta.——A. H. Garrod notes (*Proc. Zool. Soc. Lond.*, Nov. 19, 1872) some points on the *Placenta of the Hippopotamus*. The placenta is a strong cylindrical bag 3½ feet long. The bag had ruptured during parturition at the end opposite the os uteri. The outer surface of the bag is uniformly covered with villi of a bright red colour, at the closed end there is no reduction in their number, but at the ruptured end they are paler and more scattered. The umbilical cord is attached to the placenta about halfway between its two ends. It is 1½ foot long and 1½ inch in diameter in the middle, and gets larger as it approaches its attachment, near which there are many spherical bodies, as big as peas, yellow in colour, supported on short amniotic pedicles.——In the course of a description of an *early Human Embryo* in the vesicular stage of development, C. B. Reichert (*Archiv*, 1873, 127) states some facts about the *Decidua*. The embryo was believed to be at the 12th or 13th day. The ovum is completely enclosed by the decidua reflexa. The decidua vera is developed from the decidua menstrualis by the formation of cotyledonary elevations or islands in the uterine mucous membrane, and by a remarkable growth of primary and secondary papillary processes on the surface of these islands. At the margin of these islands the uterine mucosa is smooth, and shows most distinctly the widened orifices of the uterine glands. The cotyledonary walls of the d. vera form an equilateral triangle with the apex at the cervix uteri, and are separated by a median cleft into two halves; and a bilaterally symmetrical arrangement is indicated in the other islands. In the posterior wall, which alone was preserved, eight islands, more or less irregularly polyhedral and separated by furrows, were seen. The embryo was imbedded in the parenchyma of one of the islands. On separating it several small villi were drawn out of the ducts of the utricular glands. The decidua

reflexa possessed on its inner concave surface utricular glands, although the villi had especially grown from the marginal zone of the ovum; likewise a short cylindrical epithelium, free from cilia, was found here which passed into the epithelium of the utricular glands. The reflexa has only one independent wall in which the scar indicative of the closing in of the ovigerous chamber could be recognised, but in it no utricular glands could be seen: the remaining part in which the mouths of the glands were seen could not be separated by any line of demarcation from the parenchyma of the islands. The wall of the chamber next the embryo he names the basilar wall, and although in it utricular glands open, yet here, in the region of the embryonal spot of M. Coste, no villi pass into them. The view generally entertained that the reflexa is formed around the ovum resting on the vera by the elevation of a circular wall is untenable. It would appear from this specimen that the spot on the island, to which the ovum attaches itself, does not increase at the same rate as the rest of the island, in consequence of which a cup-shaped depression is formed which constitutes the basilar wall and marginal zone of the reflexa; a general growth of the free edge of the cup over the free surface of the ovum then takes place, which closes in the chamber and forms the free wall of the reflexa. The paper concludes with some observations on the stage of development of the embryo itself.

EMBRYOLOGY.—F. M. Balfour has recently published three interesting embryological memoirs (*Quart. Journ. Mic. Sc.* July, 1873). In one he relates the DEVELOPMENT AND GROWTH OF THE LAYERS OF THE BLASTODERM OF THE HEN'S EGG. He first describes the unincubated blastoderm as consisting of two layers of cells; a superficial single row of nucleated columnar cells, and a deeper layer of several rows of rounded non-nucleated granular cells, varying from $\frac{1}{1000}$ to $\frac{1}{2000}$ inch in diameter; whilst quite at the bottom of the segmentation cavity "formative cells" $\frac{1}{300}$ inch in diameter, and containing highly refracting spherules, are situated. 6 to 8 hours after incubation a hypoblast and a mesoblast are formed, whilst the superficial columnar layer forms the epiblast. The hypoblast is formed by a metamorphosis of a number of the cells of the deeper layer from their originally spherical form into flattened nucleated cells. Enclosed between the epi- and hypoblast are found numerous cells of the originally deeper layer, together with the "formative cells," which, as Paremeschko, Oellacher and Klein had previously shown, begin to travel towards the circumference and then pass in between the epi- and hypoblast. Contrary to the opinion of the above observers, Balfour believes that the cells of the mesoblast are derived, not merely from the "formative cells," but both from them and the enclosed cells of the deeper layer, by a process of conversion into new cells, though as to the exact manner of conversion he is not quite sure, but thinks that the spherules of the original cells develope into the nuclei, whilst the protoplasm of the new cells is formed from that of the original cells. The mesoblast in the chick is formed coincidentally with the hypoblast out of apparently similar segmentation cells. A kind of

fusion, as development goes on, takes place between it and the epiblast along the line of the primitive streak, to form the axis-string of His. Its growth is effected by means of the "formative cells," which rapidly increase, probably by division, and act as carriers of food from the white yolk to the mesoblast, till the formation of the vascular area, when they are no longer necessary. The growth of the hypoblast is by direct conversion, cell for cell, of the white yolk spheres into hypoblast cells. The epiblast cells increase entirely by division, and the new material is most probably derived directly from the white yolk.—— In his second memoir Balfour supports, in opposition to the opinion generally entertained by embryologists, the view originally propounded by Dursy (*Der Primitivstreif des Hühnchens*, Lahr, 1866), that THE PRIMITIVE GROOVE IS A TEMPORARY STRUCTURE, and has no connection with the development of the neural canal, or indeed with any part of the future chick. This groove is distinguished in transverse sections by the epiblast and mesoblast being fused together beneath it, by the epiblast not becoming thinner where it lines the groove, and by the mesoblast beneath it never showing any sign of being differentiated into a chorda dorsalis or other organ. Near the time when the primitive groove has reached its full length, there appears at about the 16th hour, or a little later, altogether in front of, and non-continuous with it, a thickening of the mesoblast, which forms an opaque streak, the medullary streak; and near the anterior extremity of the area pellucida arises a semicircular fold, against which the medullary streak ends abruptly. This fold is the head fold, and a groove which extends along the central line of the streak is the medullary groove, which subsequently forms the cavity of the neural canal. As the medullary groove increases in size, the primitive groove diminishes and is pushed backwards: at about 36 hours only a small and curved remnant is to be seen behind the sinus rhomboidalis, though Dursy has been able to distinguish it up to the 49th hour. The medullary groove widens and deepens, its margins, by a thickening of the mesoblast, are elevated into the medullary folds, which broaden and finally close in the neural canal. The epiblast becomes thinner where it lines the canal; there is no fusion between it and the mesoblast as beneath the primitive groove; and the notochord begins to be differentiated out of the cells of the mesoblast.—— In his third memoir Balfour describes the DEVELOPMENT OF THE BLOOD-VESSELS OF THE CHICK. He investigates the development of the blood-vessels in the area pellucida. He agrees with Klein in the fundamental fact that some of the cells of the mesoblast send out processes which unite with processes from other cells and form a network. The nuclei of the original cells divide, and at the points from which the processes start their division is especially rapid. Some nuclei acquire, especially at these points, a red colour and become red blood-corpuscles, others, together with that part of the protoplasm in which they are imbedded, become converted into an endothelium both for the processes and the masses of blood-corpuscles; the remaining protoplasm becomes fluid, and thus the original network of cells is converted into a network of hollow vessels, filled with fluid, in

which corpuscles float. From their mode of formation, the blood-corpuscles of the Sauropsida are, he believes, to be looked upon as nuclei containing nucleoli, rather than as cells containing nuclei, which would make them to be morphologically as well as functionally homologous with the mammalian red-corpuscles. This view of the formation of capillaries, by the hollowing out of an anastomosing cell network, agrees closely with the observations of Julius Arnold, of which the Reporter has given an abstract in Vol. VI. of this *Journal*, p. 437. Balfour also agrees with Klein in the mode of formation of a secondary investment of the capillaries from cells of the mesoblast, situated in the meshes of the capillary system, which flatten and send out processes, and look spindle-shaped on section. The cavity of the heart is produced by a splitting or absorption of central cells of the thickened mesoblast of the splanchnopleure, while its muscular walls are formed from the remaining cells of this thickened portion.——M. Balbiani communicates (*Ann. des Sc. Nat.* 1873) an elaborate and beautifully illustrated memoir on the EMBRYOLOGY OF THE ARANEINA.——E. Ray Lankester discusses (*Ann. Nat. Hist.*, May, 1873) the PRIMITIVE CELL-LAYERS OF THE EMBRYO as the basis of a genealogical Classification of Animals. Regarded from the stand-point of their development, animal organisms may be grouped in three classes: A. the Homoblastica, in which there is no arrangement of the cell-units into definite layers. This class coincides with the Protozoa, the Sponges being excluded. B. the Diploblastica, in which two layers of cells forming the ectoderm and endoderm remain throughout life, as the basis of histological differentiation. To this class belong the Coelenterata, including the Sponges. C. the Triploblastica possess, like B, an ectoderm or epiblast, and an endoderm or hypoblast, but a third layer of cells, or mesoblast, appears between the two. This class comprises Vermes, Echinodermata, Mollusca, Vertebrata, Arthropoda.——A. Götte makes some contributions (*Schultze's Archiv*, IX. 679) to the DEVELOPMENT OF THE VERTEBRATA. This paper contains the results of his observations on the blastoderm of the trout's egg.——K. Slavjansky describes (*Ludwig's Arbeiten*, 1872) the RETROGRESSIVE CHANGES which occur in the EPITHELIAL CELLS of the SEROUS LAYER OF THE OVUM OF THE RABBIT. The protoplasm of the cells in the first instance begins to disappear, holes form in the cells which gradually enlarge, and what remains of the protoplasm forms a branched and anastomosing network within the cell, the branches seeming to radiate from a central mass of protoplasm. The spaces between the meshes of the network are empty. In a later stage the individuality of the cells is lost by the disappearance of their boundaries, and a protoplasmic network is formed by the junction of the meshes of adjacent cells with each other. He calls the degeneration reticular, and considers it to be a physiological prototype of the pathological changes in epithelium seen by Wagner in cases of croup and diphtheria.

MALFORMATIONS.—M. Flesch describes (*Virchow's Archiv*, LVII. 289) a *malformation of the thorax*, in which there is a depression

of the sternal wall, and a diminution of the sterno-vertebral diameter of the chest, the transverse diameter is, however, increased. ——C. C. Th. Litzmann gives an account (*Archiv für Gynaekologie*, IV. 266) of a woman, aged 24, who died in child-bed, and in whom there was *extroversio vesicae*, and non-union of the pubic bones at the symphysis.——P. Gervais in his *Journal*, II. 1873, gives an account of *Polygnatic and Heterognathic Monsters*. ——R. Hein describes (*Virchow's Archiv*, LVIII. 326) a fœtus in which through defect of the anterior-wall of the abdomen there was *ectopia viscerum* and imperfect development of the extremities.——R. Jaensch describes a specimen of *pregnancy in a rudimentary uterine cornu* (*Virchow's Archiv*, LVIII. 185). The woman died about the 4th month from rupture and internal hæmorrhage, and on post-mortem examination the left uterine cornu was found to be pregnant. In this case no communication was found between the pregnant horn and the right uterus, so that it agrees closely with the two cases described by W. Turner in *Edinburgh Med. Journ.* May, 1866. The author adopts the hypothesis, supported by Turner, of the mode in which impregnation took place, viz. by an extra-uterine transmigration of the seminal fluid.

NERVOUS SYSTEM.—V. Butzke investigates (*Archiv f. Psychiatrie*, 1872, III.) the MINUTE STRUCTURE OF THE CEREBRAL CONVOLUTIONS. The nerve-cells possess a longitudinal striation, a character which the author considers to be distinctive of nerve-cells. The axile prolongation of Deiters either joins the cell directly, or through the intermediation of an offshoot from the base of the cell. He has never seen any of the other processes of the cell continued into nerve-fibres. He thinks that these processes divide, and form very fine fibrillæ, which constitute a terminal network. He also investigates the cell-forms of the Neuroglia.——Gerlach and E. Rindfleisch discuss (*Centralblatt*, 1872, 273 and 277) the mode of TERMINATION of the NERVES in the THE GREY CORTEX OF THE CEREBRUM. Gerlach holds that the non-medullated nerve-fibres of the cortex, which possess a transverse direction, form a wide-meshed network along with the radiating fibres situated in the plane of the pyramidal cells. These meshes are occupied by an extremely fine secondary network of delicate non-medullated fibrillæ. In this network the protoplasmic prolongations of the nerve-cells also end; so that it forms the medium of connection between the nerve-fibres and ganglion cells. Rindfleisch does not admit the very fine network of fibrillæ which Gerlach describes, but considers that the nerve-fibres, after a series of dichotomous divisions, terminate in the granular amorphous material of the grey matter in a penicillated ramification of very minute fibrillæ. The ends of the branched processes of the pyramidal cells also terminate, he believes, in the same amorphous material, which acts therefore as the medium of communication between nerve-cells and nerve-fibres.——Franz Boll has published (*Archiv für Psychiatrie*, IV. and separate pamphlet, Berlin, 1873) an elaborate memoir on the HISTOLOGY AND HISTOGENESIS OF THE CENTRAL

Organs of the Nervous System. In his first chapter he discusses the nature of the connective tissue of these organs, and in succeeding chapters the minute structure of the nerve-elements in the spinal cord, the white substance of the brain, and the grey matter of the cerebrum and cerebellum. He examines also the perivascular and epicerebral spaces, and the development of the central organs of the nervous system. He considers that he has established two general conclusions as to the connective tissue: first, that the view propounded originally by Henle and supported by Rindfleisch of the nervous nature of the molecular material, in which the nerve-cells, fibres and blood-vessels are imbedded, cannot be substantiated; that the "granules" (*Körner*) in that material are not, as Henle and Merkel contended, indifferent elements capable of being developed either into connective tissue or nerve-corpuscles, but are in the white substance distinct connective tissue-cells, and in the grey matter distinct nuclei, mostly with a characteristic double contour. Secondly, that there is a general unity of structure of the connective tissue in the different central organs of the nervous system. The apparent differences arise from this: that in the conversion of the protoplasm of the embryonic cells into connective tissue, a greater or less proportion of granular albuminous substance remains between the fibrillæ which arise out of the protoplasm. As there are differences in the relative proportion of the fibrillæ and the granular albuminous material in various parts, so different appearances are occasioned. The cells of Deiters which occur in the white matter of the spinal marrow of the ox, &c., are obviously only embryonal cells, which in a preponderating manner are converted into connective-tissue fibrillæ, and very slightly into granular albuminous substance. In smaller animals again these substances occur in more equal proportions. The white substance of the cerebrum is very analogous to that of the spinal marrow. Cells of Deiters, however, occur in regions such as the grey matter of the cerebrum, where the connective tissue exhibits a preponderance of the granular albuminous substance. These cells in their distribution follow closely that of the blood-vessels. He believes that under the term perivascular lymph-spaces, observers have hitherto mingled together two different kinds of hollow spaces; one adventitious lymph-spaces which have a physiological import, and communicate with the lymph-vessels of the pia; the other, the perivascular spaces which are not lymph-vessels, but artificial productions. He considers that one common anatomical principle regulates the connection of the nerve-fibres with the nerve-cells in the grey matter of the spinal cord, of the cerebral convolutions, and of the cerebellum. In each case a cell is connected with a well-marked medullated fibre, by the "axis-cylinder process," but in addition, the "branched processes" break up into delicate threads, which become continuous with a very fine nervous network, from which medullated nerve-fibres spring. He has never seen an anastomosis between ganglion cells in the sense usually expressed, viz. by a direct junction between a strong process of one cell and a corresponding process of an adjacent cell.——In *Virchow's Archiv*, LVIII. p. 255,

M. Roth communicates observations On VARICOSE HYPERTROPHY OF THE NERVE-FIBRES OF THE BRAIN; on p. 323, O. Obermeier refers to VARICOSE AXIAL CYLINDERS in the central nervous system; and on p. 310, T. Simon describes a NEW FORMATION OF BRAIN-SUBSTANCE, in the form of tumours on the surface of the cerebral convolutions.——W. Turner considers the CONVOLUTIONS OF THE HUMAN BRAIN IN RELATION TO THE INTELLIGENCE (*West Riding Asylum* Reports, 1873). He arranges his remarks under the heads mass and weight, external configuration, internal structure, vascular supply, and concludes with some observations on the organology of the convolutions. In the same *Reports*, p. 97, H. C. Major makes observations on the HISTOLOGY OF THE BRAIN IN THE INSANE, and on p. 285, W. C. S. Clapham communicates the results of the WEIGHINGS OF THE BRAIN in 716 cases.——L. Gerlach gives an account (*Ludwig's Arbeiten*, 1872) of the MYENTERIC PLEXUS OF AUERBACH. He describes ganglia in it as well as larger and smaller bands of nerve-fibres which form primary and secondary networks. From the secondary network very fine nerves arise, each of which ends in a corpuscle, which may give off one or two processes; these processes end between the smooth muscular fibres. The ganglia are very vascular.——E. Klein contributes (*Quart. Mic. Journ.* Oct. 1873) a short paper on AUERBACH'S PLEXUS in the *Intestine* of the *Frog and Toad.* In addition to the isolated groups of ganglion-cells, he describes other ganglion-cells between the circular and longitudinal muscular coats.

EYE-BALL.—A. T. Norton investigates the anatomy of the CILIARY BODY, and the accommodation of the eye to vision (*P. Roy. Soc. L.*, June 19, 1873). His paper is to show that the increase in the convexity of the lens, when accommodated for near vision, is effected by compression of its equator by the ciliary processes turged with blood, the ciliary muscle being the motor agent: the iris aids accommodation by increasing its rapidity, but accommodation can be effected slowly without the aid of the iris.——R. J. Lee communicates *P. Roy. Soc. L.*, Jan. 9, 1873, some further remarks on the SENSE OF SIGHT IN BIRDS.——A. V. Gruenhagen gives in *Schultze's Archiv*, IX. an account of the posterior surface of the IRIS.

BLOOD AND LYMPH VASCULAR SYSTEM.—Paul Langerhans contributes (*Virchow's Archiv*, LVIII. 65) to the HISTOLOGY OF THE HEART. He first describes the characters of the muscular fibre. He figures the branched muscle-cells from the heart of the Salamander, also the appearance of the transverse striation in Leuciscus, the frog and the human foetus. He points out also the relation of the nerves to the muscle-cells.——George Rolleston in the Harveian Oration (*Brit. Med. Journ.* July 5 and 12, 1873, and separate pamphlet) describes a 'MODERATOR BAND' in the HEART of the CASSOWARY, which stretches across the cavity of the right ventricle from the septum to the moveable wall of the ventricle. He points out its morphological relations to a similar band in the heart of the sheep, and to a band in the human right ventricle,

which when well developed extends from the base of the musc. papillaris arising from the outer or moveable wall of the ventricle towards the conus arteriosus.——C. Giacomini communicates a memoir on the Veins of the Inferior Extremity (*Giornale della Accad. di Med. di Torino*, 1873). Not only does he consider the usual arrangement of the veins, but he also describes cases of variation from the normal mode of disposition. He refers also to their comparative anatomy, and describes the venous system in the hind-limbs of Cercopitheci. He concludes with a chapter on some general deductions from his observations.——F. Schmuziger figures and describes (*Schultze's Archiv*, IX. 709) the Migration of the Red and White Blood-corpuscles from the mesenteric vessels of the frog.——A. Heller (*Ludwig's Arbeiten*, 1872) investigates the Blood-vessels of the small Intestine in man and several mammals. Each villus as a rule contains an unbranched artery extending to the tip of the villus. In man only does it begin to form a capillary network about the middle of the villus. The vein begins either at the tip of the villus (rabbit, man), or near the tip (rat), and passes as a rule without lateral twigs directly into the submucosa; or it arises near the base of the villus and receives lateral twigs out of the glandular layer (dog, cat, pig, hedgehog). In none of the animals examined was the plan usually described seen, of an ascending artery, a descending vein, and of a complex capillary network connecting together these vessels.——In the same *Arbeiten*, J. Michel enquires into the Blood and Lymph-channels of the Cerebral Dura Mater. The peculiarity of the anatomical arrangement of the blood-channels of the dura consists in this: that the arterial capillary net opens into two venous systems, of which the stronger is found on the outer surface, the feebler on the inner, but which communicate with each other by branches proceeding from the network on the inner surface which traverse the tissue of the dura. The space between the dura and arachnoid (arachnoid cavity) does not communicate with the vessels of the dura, specially not with the blood-vascular net on its inner surface. A system of inter-communicating spaces exists throughout the entire thickness of the dura which is in connection with the subdural space, as well as with a number of larger and smaller spaces situated between the dura and the bone, which may be called epidural; both on the inner and outer surfaces of the dura an endothelium exists, which forms the inner boundary of the epi-dural and the outer boundary of the sub-dural space, whilst the spaces within the dura are also clothed by an endothelium. This system of spaces he believes to be employed in the passage of lymph.——To the same *Arbeiten* also Schwalbe communicates a short note on the Lymph-channels of the Retina and Vitreous Body.

Respiratory System.—Sam. Pozzi records (*Revue d'Anthropologie*, 1872, 443) the case of a *right human lung* which possesses a *lobus impar*. It adheres to the inferior lobe of the lung by the inner part of its upper face, whilst the external part is free: its

inferior face is moulded on the diaphragm. It is triangular in form, and its volume is about one quarter of that of the middle lobe. He regards this lobe as a true *lobus impar* such as is seen in the generality of mammals, and as exhibiting a tendency to reversion in man to a lower type.

SKIN AND APPENDAGES. — P. Langerhans gives an account (*Schultze's Archiv*, IX. 730) of the TOUCH-CORPUSCLES AND RETE MALPIGHII. He considers the touch-corpuscle to be composed of a number of individual cells, which are characterized by the delicacy and sparing quantity of cell-substance, and between these cells throughout the entire organ the nervous elements are arranged. He disbelieves therefore in a central core, as distinguished from a peripheral substance, as well as in an investing membrane, for the cells of the periphery abut directly on the surrounding connective tissue. —— L. Stieda adversely criticises (*Schultze's Archiv*, IX. 795) Schöbl's researches on the NERVE-COILS AT THE ROOTS OF THE HAIRS of certain animals (*Report*, VII. p. 331). Stieda regards the appearance as indicative merely of different stages which mark the changes of the hair.

ALIMENTARY CANAL.—Hugo Crampe communicates (*Reichert u. du Bois Reymond's Archiv*, 1872, 569) a series of comparative observations on the LENGTH OF THE INTESTINE and extent of its mucous surface in different animals. He does not consider that the nature, *i.e.* the chemical composition, of the food exercises much influence on the structure of the digestive apparatus, but that the form in which the food is administered, by causing the organs to accommodate themselves to its volume, occasions modifications. Again, he finds variations in length even in animals born of the same mother; and that cats fed on vegetables have possessed relatively shorter intestines than cats fed exclusively on flesh. He does not believe that any constant relation exists between the weight of the body and the extent of the intestinal mucous surface.

KIDNEY.—Th. Egli gives a short account (*Schultze's Archiv*, IX. 653) of the GLANDS IN THE PELVIS OF THE KIDNEY. He was induced to examine into this subject owing to the discrepancies in the writings of anatomists, and has studied the pelvis of the kidney of the ox, pig, horse and man. The ox and pig have no glands. The mucous lining of the pelvis of the horse is studded with simple and compound tubular glands possessing a single layer of goblet and cylindrical cells. In man compound glands, intermediate in form between tubular and racemose, filled with cylinder and spindle-shaped cells, exist. The duct is very short.

COMPARATIVE ANATOMY.

QUADRUMANA. —Jas. Murie continues (*P. Zool. Soc.* June 18, 1872) his observations on the Macaques, and gives an account of Bélanger's Monkey (*M. arctoides*), of the Formosan or round-faced monkey (*M. cyclopis*), and of the Japanese Monkey (*M. speciosus*).

CARNIVORA.—A. H. Garrod notes (*Proc. Zool. Soc.* Feb. 18, 1873) certain points in the anatomy of *Arctictis binturong*. The alimentary canal, liver, lungs, genito-urinary organs and brain are described, and in some instances figured.——J. Chatin in *Ann. des Sc. Nat.* 1873, Art. 12, describes the visceral anatomy of *Viverra civetta.*

PINNEPEDIA.—B. Dybowski gives in *Reichert. u. du Bois Reymond's Archiv*, 1873, 109, a detailed description with figures of the skull of *Phoca baicalensis.*

PERISSODACTYLA.—A. H. Garrod gives an account (*Proc. Zool. Soc.* Jan. 21, 1873) of the *visceral anatomy of the Sumatran Rhinoceros.* He figures the tongue, stomach, colon and liver.——J. E. Gray records (*Ann. Nat. Hist.* May, 1873) observations on the *Dentition of Rhinoceroses*, and on the characters afforded by their skulls.

PACHYDERMATA.—J. E. Gray (*Ann. Nat. Hist.* June, 1873) makes some observations on *Pigs and their skulls*, more especially in connection with the classification of these animals.——An abstract of a memoir by W. K. Parker on the *structure and development of the skull of Sus scrofa*, is in *P. Roy. Soc. L.* June 19, 1873.

CETACEA.—A catalogue of the *Whales and Dolphins of the New Zealand Seas* has recently been drawn up by Jas. Hector (*Trans. New Zealand Inst.* v. 1872). It is illustrated with six octavo plates. ——H. S. Wilson describes in *Cambridge Univ. Reporter*, June 3, 1873, the *Rete mirabile of the Narwhal.* He showed that the rete was divisible into halves, the vessels of which were derived from two sources, and presented the same calibre at their origin. He divided arterial retia into two classes, bilateral and axial, and subdivided the axial into terminal and mediate, and each of these into complete and incomplete. The axial system he regards as fulfilling the office of supplying a large amount of blood to parts, as a means of avoiding injury from compression of vessels, and to check sudden pressure on nerve centres, whilst he believes the bilateral to act not only as a storehouse for oxygenated blood, but as a diverticulum protective against over-pressure.——J. E. Gray (*Ann. Nat. Hist.* Aug. 1873) notes some points in the anatomy of two skeletons of *Kogia Macleayii* recently received at the British Museum from Australia.—— M. du Bus describes (*Gervais' Journ. de Zoologie*, II. 97, 1873) the remains of the *Delphinidæ* found in the Antwerp Crag. He refers them to the genera *Eurhino-delphis, Prisco-delphinus, Platydelphis, Champso-delphis, Phocænopsis, Eudelphis, Hoplocetus, Palæo-delphis* and *Scaldicetus.*——P. J. Van Beneden gives in *Acad. Roy. de Belgique*, July, 1873, XXXVI., two coloured drawings of cetaceans from the Cape of Good Hope ; one he refers to the *Orca capensis* of Gray, the other he names *Lagenorhynchus de Castelnau.*——Rt. Walker in *Scottish Naturalist*, Oct. 1873, figures the lower jaw of a *common porpoise* in which conical-shaped bodies $\frac{1}{12}$ to $\frac{1}{8}$ inch in height were attached to the gum alternately between the normal teeth. A similar arrangement exists in the upper jaw. He also states that he has examined

eight or nine specimens of the porpoise caught on the coast of Scotland, all of which had tubercles on the dorsal fin. He does not regard these tubercles as Dr Gray does as specific, but only as indicating possibly a variety of porpoise.

BIRDS.—Jas. Murie describes (*Ibis*, April, 1873) and illustrates with numerous figures the UPUPIDÆ and their relationships.——A. H. Garrod (*Proc. Zool. Soc.* June 18, 1872) notes the CHARACTERS OF THE TONGUE of the psittacine genus *Nestor*. The peculiarity consists in the anterior edge of the unguis being prolonged forwards beyond the tip of the tongue for about one-tenth of an inch as a delicate crescentic fringe of hairs, which seem to result from the breaking up into fibres of the forward growing plate. The structure of the tongue leads to the placing of *Nestor* among the typical parrots and not with the *Tricho-glossinæ*.——A. H. Garrod points out (*Proc. Zool. Soc.* Jan. 7, 1873) the value in CLASSIFICATION of a peculiarity in the ANTERIOR MARGIN OF THE NASAL BONES of certain birds. In most birds the anterior margin of the nasal bone is concave with the two cornua directed forwards, which become continuous behind with the body of the bone and with one another. Birds possessing this form of nasal bone the author terms *holorhinal*. In other birds the posterior contour of the osseous external nares instead of being rounded, as in holorhinal birds, is apparently formed by the divergence of two straight bars of bone, which enclose an angular space between them. These birds he terms *schizorhinal*. Nearly all the schizorhinal birds are included among the Schizognathæ of Huxley.——M. G. Duchamp makes (*Ann. des Sc. Nat.* 1873, Art. 11) some observations on the ANATOMY OF DROMAIUS NOVÆ-HOLLANDIÆ. He describes the digestive, respiratory, urinary and genital organs, also the spleen and thyroid body.——M. Coughtrey notes (*Ann. Nat. Hist.* Sep. 1873) some peculiarities in the TRACHEAL POUCH OF AN EMEU which he had dissected.——V. Mihalkovics relates his investigations on the PECTEN IN THE EYE OF THE BIRD (*Schultze's Archiv*, IX. 591). Histologically it consists of a convoluted mass of capillaries, the spaces between which are filled with gelatinous material containing pigment. He considers that it plays an important part in the nutrition of the lens and partially of the retina.

REPTILIA.—F. Leydig describes (*Schultze's Archiv*, IX. 598) the GLANDS OF THE HEAD in the following OPHIDIANS: *Tropidonotus natrix* and *tessellatus*, *Coronella lævis*, *Coluber viridiflavus*, *Vipera verus* and *ammodytes;* and on p. 753 Leydig gives a description of the STRUCTURE OF THE SKIN of the same Ophidians.——J. E. Gray makes additional notes on the FORM of the BONES in the STERNUM of very young TORTOISES and their development (*Ann. Nat. Hist.* Oct. 1873).

AMPHIBIANS.—P. Langerhans describes (*Schultze's Archiv*, IX. 745) the SKIN of the LARVA OF SALAMANDRA MACULOSA.

FISHES.—P. Harting, by the invention of an instrument, which he calls a *physometer*, has been able to make observations on the

FUNCTIONS OF THE SWIMMING BLADDER (Abstract in *Gervais' Journ. de Zool.* II. 116). He discusses the various theories which have been propounded as to its use; as an accessory respiratory organ, or as a hydrostatic apparatus by which the fish preserves its equilibrium in the water, and refers to the experiments of Moreau, who established that the air contained in it possessed an excess of oxygen. By his instrument the dilatations and contractions of the air in the swimming bladder can be measured.——G. Gulliver (*Proc. Z. Soc.* Nov. 19, 1872) records measurements of the BLOOD CORPUSCLES OF THE SALMONIDÆ. The average sizes are as follows: *Salmo fontinalis*, Long Diam. $\frac{1}{1455}$, Short Diam. $\frac{1}{2286}$; *Salmo salar*, L. D. $\frac{1}{1524}$, S. D. $\frac{1}{2460}$, *Salmo fario*, L. D. $\frac{1}{1524}$, S. D. $\frac{1}{2900}$; *Salmo ferox*, L. D. $\frac{1}{1524}$, S. D. $\frac{1}{2900}$; *Thymallus vulgaris*, L. D. $\frac{1}{1684}$, S. D. $\frac{1}{2900}$; *Osmerus eperlanus*, L. D. $\frac{1}{2286}$, S. D. $\frac{1}{3000}$. He compares their size with that of several other osseous fishes and states that amongst osseous fishes the Salmonidæ have the largest blood-discs.——P. D. Handyside communicated to Roy. Soc. Edinburgh, Jan. 20, 1873, an account of the external characters of a new SPECIES OF POLYODON from the river Yang-tsze-kiang, and on June 2nd a description of its nervous and muscular systems.

CRUSTACEA.—E. T. Newton gives a description (*Quart. Mic. Journ.* Oct. 1873) of the EYE OF THE LOBSTER. It is illustrated with two oct. plates.——The Anatomy of the American King Crab (LIMULUS POLYPHEMUS) is described by R. Owen in *Trans. Linn. Soc.* XXVIII.

MOLLUSCA.—R. Bergh describes (*Journal des Museum Godeffroy*, Heft II.) several forms of *Nudibranchs*, and illustrates them by four large plates. The specimens belong to the genera *Phyllidia*, *Plakobranchus*, *Elysia*, *Cyerce*, *Fiona* and *Cerberilla*.

REPORT ON PHYSIOLOGY. By WILLIAM STIRLING, D. Sc., M.B., C.M., *Demonstrator of Practical Natural History in the University of Edinburgh*[1].

Nervous System.

EXPERIMENTAL RESEARCHES IN CEREBRAL PHYSIOLOGY AND PATHOLOGY.—Dr Ferrier (*Brit. Med. Journ.*, April, 1873, *West Riding Asylum Reports*, III.) has uncovered particular parts of the encephalar and Faradised them, observing the movements which follow, with a view to obtaining information regarding the source of the voluntary movements in certain parts of the surface of the brain. The results of Hitzig, obtained in an almost similar manner, have already been noticed in this *Journal*, VII. 175. Ferrier's experiments were practised upon dogs, guinea-pigs, cats, and rabbits. For the results see paper by Dr Ferrier, p. 152.——Nothnagel, vide this *Journal*, VII. 177, (*Virchow's Archiv*, 1873, LVII. 184,) finds that on making an injury, by injection of solution of chromic acid, of the diameter of 2 mm., and to the depth of 1—1½ mm. on the upper surface of the hemisphere 12—16 mm., according to the size of the animal, from the point of the hemisphere after removal of the olfactory lobes, and 2 mm. from the longitudinal fissure, one observes that the fore-leg of the opposite side, and less clearly the hind-limb, is extended forwards and outwards. This condition lasts 6—12 days, and then becomes less and less obvious, and ultimately disappears altogether. The author attributes it to a partial paralysis of the muscular sense. By irritating a particular point of the corpus striatum (nucleus caudatus), the animal begins without the least irritation from without to hop, then to rest, in a short time to hop again, always quicker, and with shorter intervals, and lastly to rush forward with great speed. The animal soon tires, and falls over, but still the legs continue to move violently. The animal could not be preserved more than eighteen hours. In *Centralblatt*, No. 35, 1873, the same author finds that in 23 cases by injuring the cortical portion of the brain with a fine needle (without injection of chromic acid), at a circumscribed spot lying near to the hinder end of the hemispheres, immediately there ensued violent spasmodic movements. The animal makes powerful jumps, and often rises ½—1 metre from the floor, the extremities then become rigid. After 1, 2, 5 minutes this passes off, the animal sits still a minute, and then hops away as if nothing had happened. ——Fournié also communicates experimental researches (*pamphlet*), Paris, 4 plates. A portion of the skull is removed and by means of a Pravaz syringe a small quantity of a saturated solution of chloride of zinc coloured blue by aniline is injected into the brain substance. We must be careful not to confound the results of small extravasa-

[1] To assist in rendering this report more complete, authors are invited to send copies of their papers to Dr Stirling, Edinburgh University.

tions into the brain substance, caused by the needle of the syringe puncturing a vessel, with those of the injection. The dog was the animal operated upon. In 5 experiments out of 7 a complete abolition of the sense of touch was produced, which coincided with the complete destruction of the thalamus opticus. The author seeks to explain this by the relatively large commissure which unites the two thal. optici in the dog. The *thalamus opticus*, the author thinks, furnishes the conditions for a 'perception simple' (he does not say 'avec connaissance'). With regard to lesions affecting motion, by far the most salient and pronounced is the 'mouvement de galop,' which is produced in one and the same animal incessantly till just before death, when, according to the intensity or seat of the lesion, these movements are followed by paralysis more or less complete. These movements the author attributes to the effect of the caustic upon the fibres which pass between the optic thalamus and the corpus striatum. The paralysis appears because the sensitive parts of the thalamus are destroyed by the injection, and, in consequence, cannot excite the motor cells of the corpus striatum. Lesion of the white substance below and to the inner side of the thalamus, and which connects it with the corpus striatum, is followed by paralysis of the hind extremities. The respiration is very peculiar, being rapid and deep. *Corpus striatum;* as in the thalamus, lesion of this organ on *one* side produces the same phenomena as if both had been injured. As a general rule, no affection of sensibility was observed, while paralysis of motion had a constant occurrence. The tendency presented by the animal to move in a circle, or to turn to the injured side, the author observes, are not characteristic of lesion of the corpus striatum, for they also follow injury to the white substance of the brain, and of the convolutions. Lesions of the *cervical portion of the brain* at the anterior, middle, or posterior parts, did not extinguish simple perceptions, while, on the contrary, the absence of intelligence ("connaissance") and memory was constantly observed. Regarding motions, two periods were constantly observed, the one a period of excitation when the animal ran, or moved in a circle &c., and the other a period of prostration, and sometimes of paralysis. The first period the author thinks corresponds to the simple excitant action of the caustic, while the latter is referable to a destruction of the tissues by the same. Injury to the *white matter of the brain* was followed by the same phenomena which accompany lesion of the convolutions, optic thalamus, and corpus striatum, and this because the fibres which compose the white part of the brain serve to bind, on the one hand, the thalamus with the peripheral part of the brain, and on the other, the peripheral part of the brain with the corpus striatum. Lesion of the *cerebellum* produced phenomena of excitement; the animals ran about wildly, assumed ridiculous positions, while the eyeballs were always turned from below upwards, &c. This state was succeeded by a period of collapse, characterised by paralysis. More or less complete sensibility was profoundly affected in the greater number of these lesions. Then follow the results of a series of complex lesions, not referable distinctly to injury of any one part of the brain, be-

cause several parts of the brain had sustained injury. Injury of the *cornu ammonis* was followed by the same phenomena as lesion of the other convolutions, but, what was very peculiar, the animal without being paralysed was unable to stand upon its four feet, being only able to crawl along on its belly. It had lost the sense of equilibrium. See also a paper communicated by the author to the *Académie de Médicine*, of which an abstract is given in the *Lond. Med. Rec.*, No. 35.

COMPOSITION OF THE GREY AND WHITE MATTER OF THE BRAIN.—*Pflüger's Archiv*, 1873, VII. 367. Petrowsky, under Hoppe-Seyler's direction, has investigated the chemical composition of the grey and white matter of the brain. The following represents the composition of the brain of the Ox.

	Water.	Solids.
100 grms. of grey substance contain	81·6042	18·3958
100 grms. of white substance contain	68·3508	31·6492.

100 grms. of Solids.	Gray.	White.
Albuminous compounds and Glutin	55·3733	24·7252
Lecithin	17·2402	9·9045
Cholestrine and Fats . . .	18·6845	51·9088
Cerebrin	0·5331	9·5472
Substances insoluble in pure ether	6·7135	3·3421
Salts	1·4552	0·5719.

CHEMICAL REACTION OF THE CENTRAL NERVOUS SYSTEM.—R. Gscheidlen (*Pflüg. Archiv*, VIII. 171) experimented upon the brains of horses, dogs, rabbits, &c. An incision was made into the brain, and its reaction tested by means of Liebreich's plates (*Berichte d. Chem Gesell. zu Berlin*, 1868, p. 48). The grey substance always showed an acid reaction, while the white reacted always neutral, or slightly alkaline. These results were constant in more than 70 animals, and it was immaterial whether the animals were poisoned with morphia, or curara, or whether they had served for a longer or shorter time for experiments, or had been killed at once. These results are not due to post-mortem changes. The same results were obtained on the ganglia of the sympathetic, which exhibited an acid reaction, while the connecting nerve-fibres were neutral or slightly alkaline. The same also is true of the grey and white substance of the spinal cord. The author thinks that the ganglion cells contain as a constituent a free acid, which is in all probability lactic acid. After death the acid reaction of the grey substance increases to a certain degree, while in the white this reaction occurs quite as little as in peripheral nerves (vide Heidenhain's observations). If the white substance shows an acid reaction, it is due to the passage of the acid into it from the grey matter, for if in a large animal the gray and white matter are kept apart, the reaction of the white remains alkaline or neutral, the grey of course acid.

EMBRYONAL DEVELOPMENT OF NERVE-CELLS.—Lubimoff (*Centralblatt*, No. 41, 1873) has studied the development of the nerve-cells

in the human embryo. He found that the cells of the sympathetic system reach perfection sooner than those of the central part of the cerebro-spinal nervous system. In the sympathetic system itself these cells, which are found in the branches of the cerebro-spinal nervous system, reach perfection sooner than those of the pre-vertebral cords and the ganglion cœliacum. In the central cerebro-spinal system the cells of the spinal cord (earliest the nerve-cells of the anterior horn) are sooner perfected than those of the grey substance of the cerebrum and cerebellum.

Transmission of Reflex Impressions in the Spinal Cord.—Rosenthal (*Bericht der K. Akad. der Wissensch.* Berlin, February, 1873. Abstract by Dr Brunton, in *London Med. Record*, No. 29) finds that a certain time is required for the transmission of a sensory impression to a motor nerve. This time (reflex time) depends on the strength of the irritation—the stronger the irritation the shorter the time required. A longer time is required for the transmission of a sensory impression made on one side of the body to the muscles of the other side, than to the muscles on the same side. This difference also depends upon the strength of the irritation—the stronger the irritation the shorter the time. In peripheral motor nerves no dependence of the rapidity of conduction on the strength of the irritation can be observed. The nearer the irritated spot is to the spinal cord, the more easily is the reflex time diminished. The reflex time is altered by the exhaustion of the spinal cord. When an adequate irritation is applied to an exposed sensory nerve, at two points as far apart as possible, the reflex time for the point furthest away from the cord is greater than for the nearer one.

Reflex Relations of the Stomach to the Nerve Centres of the Circulation.—*Sitz.-berich. der Wien Akad. der Wissensch.* lxvi. 1872. *Centralblatt*, No. 13, 1873. Mayer and Pribram have experimented upon dogs and cats. The experiments of Goltz on irritating the walls of the stomach have shown that this is followed by slowing of the pulse, but these authors have found that there is an increase in the arterial pressure. The results were the same both by mechanical and electrical irritation, or by insufflation of the stomach by means of an india-rubber bag. In opposition to the experiments of Hermann and Ganz, these authors state that neither by injection of cold water, nor by pieces of ice introduced into the stomach, could they observe any effect. It seems, from these experiments, that irritation of the mucous membrane alone has no effect on the pulse or blood-pressure; but the irritation to produce these effects must extend to the serous and muscular layers.

Reflex Paralysis of Vessels and Affections of the Spinal Cord after Suppression of the Perspiration by varnishing animals.—Feinberg (*Centralblatt*, No. 35, 1873) finds that in animals coated with varnish the greater part of the morbid phenomena are spinal-cord affections. To be observed are tremor, hyperæsthesia,

later partial anæsthesia, enacted reflex sensibility, reflex and tetanic spasms, rotation round the axis (observed three times), paralysis of the bladder, &c. Frequency of respiration decreased, action of the heart lessened, and the temperature diminished. All these phenomena are of different duration and different intensity. If the animal dies rapidly the period of irritation is very short, hyperæsthesia and reflex irritability are not very pronounced, and merge quickly into partial anæsthesia and paralysis. Spasms never fail. In fact, all the phenomena of depression are present. When death takes place more slowly, hyperæsthesia and reflex irritability are so intense that the slightest contact with the skin, a stamp of the foot on the ground, is followed by a succession of powerful reflex spasms; the other phenomena appear later, some time before death. The temperature depends upon the unhindered or hindered radiation of heat. In the first case it sinks quickly and progressively till 19 — 20° C. In the second case it exceeds or differs little from the normal, and then just before death it begins quickly or slowly to sink, then death quickly sets in. The post-mortem phenomena observed were: Dilatation of the sub-cutaneous vessels, which were injected; large extravasations from the pulmonary capillaries, not seldom also under the pleura, heart-chambers over filled with blood, often extravasations in the heart-substance, distension of the roots of the portal veins, and extravasations in the liver-substance; the mucous membrane of the stomach constantly exhibited extravasations; strong injection of all the capillaries of the serous layer of the intestine, with catarrh of its mucous lining; kidneys filled with blood, with commencing parenchymatous inflammation, capillaries of the peripheral nerves strongly injected, often with extravasations, the same in the voluntary muscles. Membranes of brain injected. Gray substance of the cervical spinal cord dark red, with small extravasations. Dorsal and lumbar regions of the cord less injected. Microscopical examination shows in the cervical part of the spinal cord numerous capillary extravasations with partial destruction of its substance, proliferation of neuroglia less obvious in those animals which died quickly, but strongly developed in those which died slowly. The nerve-fibres were compressed and atrophied. All these phenomena were more pronounced in the cervical part of the spinal cord than lower down. These phenomena can only be explained by a paralysis of all the vaso-motor nerves in the cervical part of the spinal cord or their centre in the med. oblongata.

Influence de l'Activité Réflexe des Centres Nerveux Vasculaires sur la Dilatation des Artères Périphériques et sur la Sécretion des Glandes sous-Maxillaires.—Owsjannikow and Tschiriew (*Bull. de l'Acad. Imp. des Sc. de St Pétersbourg*, XVIII. 18, Mai 1872. Abst. in *Arch. de Phys.* 1873, No. 1, p. 90) find that irritation of the central end of the N. ischiadicus with intact chorda tympani is followed by secretion of saliva from the submaxillary gland. These authors explain this as due to the increased blood-pressure (from irritation of a sensory nerve) in the body gene-

rally and in the gland. Irritation of the N. ischiadicus when the chorda is cut diminishes the secretion. Irritation of the ganglion sub-maxillare can diminish a rapid secretion. These authors suppose that there is in the chorda tympani fibres which widen the vessels (secretion fibres), and from the ganglion sub-maxillare proceed fibres which narrow the vessels (fibres which diminish the secretion). Irritation of the peripheral end of the N. splanchnicus (which is followed by increased blood-pressure) produced the same result on the secretion as irritation of the N. ischiadicus.——Grützner and Chtapowski (*Pflüg. Arch.* VII. p. 522) have repeated the above experiments, and find exactly as the previous authors, that on irritating the central end of the N. ischiadicus the secretion from the canula fell drop for drop with a frequency scarcely inferior to that obtained by directly irritating the chorda. The saliva also was the so-called chorda saliva. On irritating the left N. ischiadicus and cutting the left chorda (canula in both ducts) the right sub-maxillary glands secreted in the old way, while the left yielded very little or almost no secretion. The blood-pressure was measured in the art. cruralis. These authors explain the fact why irritation of a sensory nerve, with cut chorda, produces now and again a small outflow of saliva thus. They say it is owing to the pressure of the contraction of the muscles which pass over these glands. These muscles can contract (in an animal not too powerfully poisoned with curara) quite as well as the other muscles of the body. Heidenhain has shown that atropia paralyses the secretory fibres of the chorda, while the inhibitory fibres are intact, and Ludwig has shown that the increase of blood-pressure and increase of secretion do not stand in the relation of cause and effect. To determine whether the increase of secretion observed on irritating the N. ischiadicus was really due to increase of the blood-pressure, or was only a reflex secretion, these authors injected atropia (0·001—0·005) into a vein in a curarised dog, and found that on strong irritation of the N. ischiadicus no secretion was obtained from the canula, but red blood often at rhythmical intervals flowed from the vein of the gland, as ought to happen on peripheral irritation of the chorda. The vaso-motor nerves of this nerve were here reflexly stimulated, and their section brought the whole to an end, for then there is increase of blood-pressure in the vessels of the gland, and no secretion. The increased salivary secretion on irritation of the N. ischiadicus is therefore produced by a reflex action on the secreting fibres of the chorda. From these experiments these authors think that the fibres of the chorda, as well as of the sympathetic, have their origin in the medulla oblongata.

INNERVATION OF THE EAR OF THE RABBIT.—Moreau states in *Arch. de Phys.* IV. 667, that section of the sympathetic alone produces redness of the under half of the ear due to widening of the vessels; while section of the N. auriculus magnus (cervical plexus) produces a similar effect on the upper half. It is therefore obvious that both nerves are concerned in the innervation of the vessels of the ear, and that the contraction of the vessels of the under half of

the ear depends upon the sympathetic, while that of the upper depends on the N. auriculus.——"Functions of the Lingual Nerve." Prévost, in *Arch. de Phys.* v. 1873, 253 and 375.——"Contributions to Physiology of the Vagus." Arloing and Trissier in *Arch. de Phys.* v. 1873, p. 157.

Reasons for the pathological changes in the Lungs after section of both Vagi.—Alf. Genzmer in *Pflüg. Archiv*, viii. 101. The author concludes from his experiments (1) that the altered action of the heart produced by paralysis of the vagus is without influence on the lung-tissue. (2) Hindering of the supply of blood. (3) Forced passage of saliva does not produce disease, as was found after section of both vagi. (4) Paralysis of the vagus can remain without visible consequences on the condition of the lung-tissues, it however increases their tendency to disease, and that more so on the paralysed side. (5) Paralysis of both pulmonary vagi produces a neuro-paralytic hyperæmia of the lungs. (6) If saliva is forced into the lungs, rendered hyperæmic (œdematous) through section of the vagus, it produces inflammation.

Innervation of the Spleen and its relation to Leucocythæmia.—Tarchanoff in *Pflüg. Archiv*, viii. 97 says, that in a curarised dog with artificial respiration kept up and blood-pressure measured in carotid, he obtained the following results. (1) Irritation of the central end of the vagus (medium and strong) for a minute or more, produced, besides the increase of blood-pressure, a powerful contraction of the spleen, about 1—2 c.m., while irritation of the peripheral end produced a scarcely perceptible contraction, or no effect at all. (2) Irritation of the central end of the N. ischiadicus produced, besides the increase of blood-pressure, a contraction of the spleen, but less than in the above case. (3) Irritation of the medulla produced a very powerful contraction of the spleen about 2½ c.m., but this only when the N. splanchn. are intact, for these nerves contain the centrifugal nerves for the vessels of the spleen. This contraction does not disappear on cessation of the irritation, but the spleen returns gradually to its former size, whilst the blood-pressure has already returned to its original height. (4) Gradual section of the nerves of the spleen, as is known, is followed by swelling of the organ to one-and-half times the original size. After section of the nerves of the spleen the blood taken from the spleen in the 2nd, 3rd, and 4th days after the operation showed a quantity of white blood-corpuscles in the field of the microscope, varying from 60 to 70, whilst the blood taken from the ear before the experiment showed only from 6 to 15. Leucocythæmia so produced however gradually disappears, and at the end of a week the number of white blood-corpuscles is normal. Parallel with this phenomenon is the diminution of the size of the spleen. Vide also "Physiology of the Spleen," by Bochefontaine in *Arch. de Phys.* v. 558.——"Innervation of Blood-Vessels," by E. Pick in *Reich. und Du Bois Rey. Arch.* 1873, 1. Abstract in *Centralblatt*, No. 46, 1873.

ON THE CENTRE OF THE NERVES OF ERECTION.—Goltz, *Pflüg. Archiv*, VII. 582. Eckhard says that the nervi erigentes can be followed in the interior of the spinal cord to the pons varolii and cerebrum. The author, from experiments upon dogs, arrives at a different conclusion, viz. that the nearest centre from which the nervi erigentes spring is placed in the lumbar portion of the spinal cord. Upon section of the posterior part of the thoracic part of the spinal cord erection could be produced in a reflex manner. Further, that the condition of the reflex can be hindered by irritation of sensory nerves. He has observed peculiar reflex rhythmical contractions of the sphincter ani in animals with divided spinal cord. The lumbar spinal cord is a more important centre for reflex movements and reflex inhibition than was formerly supposed.

Hermann, "Action of galvanic currents on Muscles and Nerves." *Pflüg. Archiv*, VI. 312 and 561. Abstract by Rosenthal, *Centralblatt*, No. 31, 1873.——Grünhagen, "On the secondary Contractions in Muscle." *Pflüg. Archiv*, VI. 119, 1872. Abstract in *Centralblatt*, No. 18, 1873.——Grünhagen and Englemann, "Intermittent Irritation of Nerves and Muscles." *Pflüg. Archiv*, VI. 157, 1872. Abstract in *Centralblatt*, Nos. 19 and 20, 1873.——Vulpian, "Influence of traumatic Lesions of Nerves on the physiological properties and the structure of Muscles." *Arch. de Phys.* IV. 245. Abstract in *Centralblatt*, No. 15, 1873.——"Transverse Conduction in Frog's Nerves." Hitzig in *Pflüg. Archiv*, VII. 263.——"On the Form of the Contraction in the so-called transverse Conduction in Frog's Nerves." Wm. Filehne in *Pflüg. Archiv*, VIII. 71.——"Influence of Section of a Nerve on the nutrition and regeneration of the Tissues." Schulz in *Centralblatt*, No. 45, 1873.——"Alterations of the Spinal Cord after extraction of the Sciatic Nerve in the Rabbit." G. Hayem in *Arch. de Phys.* V. 504.——"Union of the cut end of the Lingual and Hypoglossal Nerves." Vulpian in *Arch. de Phys.* V. 597.——"Electrical Irritation of Nerves." Fick in *Arbeiten aus d. Physiol. Lab. d. Wurzbürger Hochschule*, Pt. I. 65.——Ranvier, "Degeneration of Nerves after Section" in *Comptes Rendus*, LXXV. 1831; and "Researches on the Histology and Physiology of Nerves," in *Archiv. de Phys. et Pathol.* IV. 427, 1872.——"Irritation of Nerves by electric currents." *Zeitsch. f. Biolog.* VIII. 71 and 100, Schiff, Fuchs.——Englemann, *Nederl. Arch. S. A.* 5. Abstract in *Centralblatt*, No. 33, 1873.——"Action of very gradual thermal irritation on the sensibility of Nerves." Heinzmann in *Pflüg. Archiv*, VI. 222.——"Regeneration of Nerves in Paraplegic Animals." Prévost and Waller in *Gaz. Méd. de Paris*, 1873, No. 10. These authors find in rats and guinea-pigs, rendered paraplegic by section of the spinal cord, that degeneration and regeneration of the cut or pinched sciatic nerve takes place just as in the sound animals.

CONDITION OF THE SKIN UNDER SLIGHT MECHANICAL IRRITATION.—*Centralblatt*, No. 26, 1873. Petrowsky remarks that the phenomenon of white stripes in the skin, observed by Bouchert in Scarlatina, and by Bäumler in other febrile disorders, Exanthematic Typhus,

Pneumonia, Traumatic fever (*Centralblatt*, No. 12, 1873), he has found also to take place in the normal skin. Make a line with the finger-nail gently upon the skin, at first at the irritated spot nothing is observed, but after the lapse of $\frac{1}{4}$—$\frac{1}{2}$ min. the irritated line begins by degrees to grow pale, the paleness rapidly reaches its maximum, persists for a short time, and then disappears by degrees. The duration of the phenomenon, until it has completely disappeared, varies from 4—6 minutes, and depends upon the strength of the irritant employed, and also on the individual. This condition can be produced on a dry skin as well as one wet with perspiration. The author believes that the phenomenon is probably due to contraction of the cutaneous capillaries, and that other histological elements of the skin are also active.

CHORDA TYMPANI.—Vulpian has continued his researches on this nerve, and has communicated his results to the Société de Biologie. The observation of Heidenhain is confirmed, that in a dog brought under the influence of atropia, excitation of this nerve is no longer followed by increased sub-maxillary secretion, but further dilatation of the vessels of the glands, and that in excitation of the lingual nerve the vessels dilated just as if no atropia had been administered.

VASO-MOTOR ACTION OF THE SPLANCHNIC NERVE.—Vulpian (*Gaz. hebdom.* 1873, No. 21). Section of this nerve 3 cm. above the left suprarenal body, as performed on a curarised dog. The volume of the kidney on the same side and the quantity of blood it contained was increased. The urine albuminous and diminished in quantity, but not containing blood or epithelial cells from the tubuli. By irritating the peripheral end of the nerve by an induced current the kidney became pale, the veins contracted and the secretion of urine ceased.

EYE.—"Keratitis after Section of the Trigeminus," Eberth, in *Centralblatt*, No. 32, 1873.——"Section of the Optic Nerve," Berlin, in *Centralblatt*, No. 28, 1873.——"Influence of the Sympathetic upon the Eye," Eckhard in *Centralblatt*, No. 35, 1873.——"Visible Direction," J. Jago, in *Proc. Roy. Soc. Lond.* XXI. 213.——"On the Physiological Action of Light," by Jas. Dewar and J. G. M. Kendrick, in *Journ. of Anat. and Phys.* June, 1873.

EAR.—"Sensibility to Sound," Müller, in *Ludwig's Arbeiten*, VI. p. 1.——H. Buck and Burnett, "Experiments on the Mechanism of the Auditory Ossicles," in *Centralblatt*, No. 17, 1873.——Rumbold, "On Functions of the Eustachian Tube," in *Lond. Med. Rec.* No. 29.——Rüdinger, on "Closure of the Eustachian Tube," *Ibid.* No. 18.——"Perception of high Musical Notes," in *Boston Med. and Surg. Journ.* 1872, X. 20.——"Accommodation of the Ear," Mach and Kessel, Abstract in *Lond. Med. Rec.* No. 37, 1873.——"On the Movements following Section of the Semicircular Canals." Böttcher (*Dorpat. Med. Zeitschr.* 1872, III. 97. Abstract in *Centralblatt*, No. 5, 1873) in his experiments has used frogs, because he found that by

careful dissection he could expose these canals without loss of blood, and without injury or disturbance to any of the surrounding important parts. In opposition to the results of Flourens and Goltz, B. finds that no disturbance of equilibrium was produced by this injury, whether it was performed on one or both sides. These results are in direct opposition to those of Löwenberg (*Arch. f. Augen- u. Ohrenheilk.* 1872, III. Abstract in *Centralblatt*, No. 18, 1873), who operated upon pigeons, and by a modification in the method of operating, the author wished to determine whether the movements were caused by pain as supposed by Flourens, and whether the presence of consciousness was necessary for these disturbed movements, and whether the cause consists in an irritation or paralysis of nerves. The results of his experiments are: (1) The disturbance of movement recurring after section of the semicircular canals of the ear depends only upon this injury and not from any injury to the brain. (2) The vomiting observed by Czermak in his experiments depends upon injury of the cerebellum. (3) The disturbance of movement is the consequence of irritation of the membranous canals and not of paralysis of the same. (4) The irritation of the canals produces the spasmodic movements in a reflex manner without the cooperation of consciousness. (5) The transference of this reflex irritation of the nerves of the membranous canals to the motor nerves takes place in the thalamus opticus. With regard to the statement of Brown-Séquard, that section of the N. acusticus produces these movements, the author, operating upon rabbits, finds that section of this nerve from the tympanum yields the same results, but in this case the semicircular canals are at the same time partly pinched and partly pierced.

Skin.

EXCRETION OF CO_2 BY THE SKIN IN MAN.—Aubert (*Pflüg. Archiv*, VI. Hf. ii.) has determined by a special apparatus the amount of CO_2 excreted by the skin of a healthy man per diem. The subject experimented upon was made to sit in a box which fits tightly round the neck, and through which air is gently passed. In 24 hours a maximum amount of 6·3 grms. and a minimum of 2·3 grms. was eliminated by the skin of the whole body minus the head. This gives a mean of 3·87 grms., while the quantity discharged by the lungs daily is 14,000 grms.

BACTERIA IN SWEAT.—Eberth (*Centralblatt*, No. 20, 1873) finds that in ordinary as well as in yellow sweat there are multitudes of bacteria. These bacteria are small, oval, united in strings of twos and threes and endowed with living movements. In spots covered with hair they attach themselves to the hair and even penetrate into the hair, which then splits and breaks.

Circulatory System.

"Circulation and its Disturbances in the Frog's Lung."—Hüter in *Centralblatt*, Nos. 5 and 6, 1873.——"Physiological Action of

Digitalis on the Circulation and Temperature." Abstract in *Centralblatt*, No. 13, also Brunton and Meyer, *Journal of Anat. and Phys.* VII. Nov. 134, 1872.——"On the Physiology of the Circulation in Plants, in the Lower Animals and in Man." Lectures by Pettigrew, *Edinb. Med. Journ.* 1872—73.——"Law which Regulates the Frequency of the Pulse." Garrod in *Journ. Anat. and Phys.* VII. 219.——"On the Work of the Frog's Heart under varying Blood Pressure." Blasius in *Arb. aus d. Phys. Lab. d. Würzburger Hochschule*, Pt. I. p. 1 and "On the Variation of the Blood Pressure in different parts of the Circulatory System," by Fick, in *ibid.* Pt. II. p. 183.

PRESSURE IN THE PERICARDIUM.—Adam Kiewiez and Jacobson (*Centralblatt*, No. 31, 1873) have measured in living animals the intra-pericardial pressure by means of a small trocar with a short stiletto point introduced through the 4th intercostal space at the left margin of the sternum into the pericardium. On sheep, dogs and rabbits, when the experiment succeeded, i.e. when the pericardium was perforated with no loss of blood and no injury to the cardiac muscles, they found without exception a negative pressure within the pericardium. The negative pressure varied between -3 and -5 mm. Hg. By gentle respiration they never saw this bound exceeded, but in the rabbit with artificially induced dyspnœa, they observed it to fall to -9 mm. Hg. The force with which the venous blood is drawn to the heart is less important than is generally believed. Donders estimated it in the respiratory pause at about $7\frac{1}{2}$ m., after ordinary inspiration at least 9, after deepest 30 mm. Hg. These authors' results are a half lower.

CIRCULATION IN THE CORONARY ARTERY.—*Archives de Médicine*, Feb. 1873. M. Rabatel has ascertained that in the horse the current of the blood in the coronary artery undergoes a double variation. The first increase strongly pronounced, and coincident with the ventricular systole, is due to the contraction of the ventricle; the second coincident with the diastole is explained by the facility with which the blood passes through the peripheral capillaries.

ON THE CONDITION OF THE BLOOD STREAM AFTER LIGATURE OF THE VENA PORTÆ.—H. Tappeiner (*Ludwig's Arbeiten*, VII. p. 11). It has long been known that rabbits die very soon after ligature of the vena portæ, and with a great decrease in the arterial pressure. The decrease of arterial pressure and the death of the animal have been explained by the previous authors (C. Ludwig and Thiry) upon this subject as due to the accumulation of such a large quantity of blood in the rootlets of the vena portæ so that there is not sufficient blood in the other vessels and parts of the body to carry on the circulation. F. Hofmann and the author have found that after ligature of the vena portæ and death had resulted, in a rabbit the quantity of blood which had accumulated in the rootlets of the vena portæ was only 0·8 per cent. of the weight of the animal. By performing experiments with blood-letting the author has found that the abstraction of a quantity of blood equal to 1·3 per cent. of the weight of the

body does not cause the death of the animal, and gives quite other changes in the arterial pressure. The author thus shows that the explanation of the previous authors is not tenable. Studying the curves of decreasing blood-pressure during the time of ligature of the vena portæ, the author has found that the decrease takes place regularly in cases where the animal remains quite quiet. This decrease however is not regular when muscular contractions and irritation in the region of the vaso-motor nerves are present. The variations by which the heart-beats and respiration are represented in the arterial curves become after ligature of the vena portæ at once flatter, and then disappear altogether when the arterial pressure has already clearly decreased. After removal of the ligature in some cases, the increase in arterial pressure begins and rises until the tension that was present earlier is reached. In other cases the arterial pressure may remain low for some seconds or even minutes before it begins to rise, which it does at first gradually, then more quickly. Active or passive movements during the ascent accelerate clearly the increase of the pressure already present. At other times removal of the ligature is not followed by a spontaneous increase in the arterial pressure. With regard to the pulse-beats, in all cases the initial diminished frequence increases with the duration of the experiment, so that when by the continued occlusion the animal is brought near to death, there is a period in the middle between the initial slowing and that induced later by dying, in which the pulse is accelerated. Section and irritation of the spinal cord during ligature of the vena portæ produces an increase of the arterial pressure, and the waves of the pulse are to be seen more clearly. Studying the rapidity of the movements of the blood in the carotid when the vena portæ is ligatured, the author found that it is always decreased. For a description of the apparatus by which the above results were obtained, we must refer to the original.

Uniformity of the Heart's Work when the Organ is not subject to any Exterior Nervous Influence.—Marey (*Comptes Rendus*, Aug. 4, 1873, p. 367) seeks to establish the theory that (the innervation remaining constant) the heart which never rests executes an amount of work sensibly uniform, its beats being rare when each of them has to overcome a considerable resistance; they are frequent, on the contrary, when that resistance diminishes. The resistance to the effort of the heart is the pressure of the blood already contained in the arteries. He then cites a number of facts by which he supports his theory. With regard to the experiments of Cyon with the depressor nerve, the heart is in this case influenced by the nervous system. Irritation of the central end of this nerve produces a retardation of the heart's beats and consecutive diminution of the arterial pressure. Certain facts, the author remarks, seem to stand in opposition to his theory, viz. that even when in three rabbits Cyon had destroyed all the nerves which run along the vessels, so as to isolate the heart from all external nervous influence; on the depressor nerve being excised under these conditions, retardation of the heart-beats

followed. The author thinks that some nerve-fibres of the pneumogastric may not have escaped the scalpel. Ludwig has shown that the excised heart of a frog (and even the heart of a rabbit) when filled with serum, will continue its movements for a long time, and Bowditch, Coats and Cyon have measured with a manometer the energy of its movements. Such an excised heart (Tortoise), free from all external nervous influence, was fitted with an artificial circulatory apparatus formed of caoutchouc tubes, in which circulated fresh calf's blood. Blood was introduced into the veins and auricles from a raised reservoir by means of a syphon, passing from the ventricles to the arteries, the blood was forced into the elastic tubes, and was again poured into the reservoir. In spite of a high temperature, this circulation was maintained over five hours, and the following experiment was repeated a great number of times. Every time upon contracting the orifice of outflow of the arterial blood, only raising it to a greater or less height, the pressure in the arteries thereby being increased, the heart's movements were retarded. Every time on the contrary on diminution of the pressure, the heart-beats were accelerated. In the absence of all communication with the nervous centre, the heart beats more quickly, in proportion as it performs less work in each of its beats.

A Periodic Function of the Isolated Frog's Heart.—L. Luciani (*Ludwig's Arbeiten*, vii. 113). When the author ligatured the auricles of a frog's heart, into whose ventricle a canula had been introduced through the sinus venosus, and thereafter the excised heart filled with serum and brought into connection with a small registering manometer, he observed, instead of the long pause in which the heart generally remains after the method of ligaturing of Stannius, that a long series of pulsations followed in a very characteristic, intermittent manner. To study more exactly this function the author employed the method of Bowditch (*Ludwig's Arbeiten*, vi. 139), with certain modifications, for a description of which we must refer to the original. By means of a canula, which had at one end five small rings adjoining each other (each ring 2 mm. broad), the author could ligature the ventricle at any height he wished. This intermittent pulse-rhythm shows itself in every frog's heart filled with serum, by a paroxysm ("Anfall") of many frequent beats, which is then followed soon, or after a longer pause, by a series of slower beats. Then the groups ("Gruppen") first appear, consisting of two and more seldom sixty contractions (when the serum contains many blood corpuscles), which are more frequent towards the middle of the group than at the beginning or end of the same. Pauses of from 20 seconds to 10 minutes separate the groups. In one and the same heart however there is often a remarkable regularity in the proportion of the groups and pauses. When such periodic work has lasted for a long time (often hours) the groups, which are already chiefly composed of lower beats, become dissolved, and merge into isolated contractions ("Krisis"). The act of ligature produces often (especially on ligaturing near the sulcus) a very high and long

(seldom more than 9 minutes) continued tonic contraction of the heart, upon whose curve often single beats are marked, sometimes arranged in groups separated by pauses. On loosening the ligature the tonus disappears immediately. Ligature of the places tied cannot again produce these results; the periods therefore following the tonic paroxysm are not to be regarded as phenomena of irritation, but as consequences of the separation of the centres regulating the beats. The influence of the serum is shown in that upon its renewal it makes the beats in the groups higher and more frequent and the pauses shorter. Warming of the heart from 24° to 30° Cels. makes the groups and pauses shorter, the pulsation higher and much quicker, so that the rapidly widening heart causes the mercurial column in the manometer to oscillate under the abscissa; cooling to 3° Cels. produces an opposite effect. The single beat lasts often more than four seconds. The height of the beats following each other diminishes very gradually. The absolute number of beats in a given time under all the above conditions is smaller when the beats are arranged in groups than when they follow each other in the normal manner, but are equal (to those of the uninjured heart) with regard to height and duration. The pauses are longer in a heart beating in oil than in one filled and bathed with serum. One can therefore conclude that during the pause the irritation is actually absent, not, however, that its manifestation is only suppressed by the increased resistance. In the course of a normal pause, if contractions are produced either by mechanical or electrical stimulation the pause is thereby increased. The easily changed rhythm of the periodic acting heart is easily affected by poisons. Nicotine and atropia make the groups longer, the pauses shorter. The latter, however, in small doses causes death of the heart without dissolution of the groups in the period of "Krisis." Large doses cause at first a long series of frequent then more seldom beats, whilst nicotine makes the pauses shorter, and produces "Krisis" more rapidly without disturbing the energy of the heart. Whilst new serum could not reproduce the periods after atropia poisoning, this happened in most instances after strong poisoning with nicotine. A heart poisoned with nicotine that had already passed through all the phases from "Anfall" to "Krisis" could be brought to a new periodic action when it was either filled with new serum or again poisoned with nicotine. After a strong dose of extract of muscarin (5 milligr.) the heart remained motionless (as already known) after it had made a short group (sometimes peristaltic) of beats, but it reacted upon mechanical and electrical irritation. Careful washing out of the heart with fresh serum produced no spontaneous beats. A new ligature produced tetanus, upon whose curve single beats were marked, but immediately after the heart again relapsed into rest. Medium doses (2 milligr. ext. musc.) make the groups shorter, the beats more seldom and smaller, and the pauses longer. Minimum doses of muscarin (0·05 milligr. of the sulphate), after a stage of excitation, in which the height and frequency of the beats are increased, appear to produce the depressing effects of medium doses.

Atropin produced its effect regularly upon the muscarin poisoned heart as well as on the normal one. On the contrary, nicotine cannot annul the action of muscarin. All the facts seem to the author to render the idea untenable, that there are present in the heart separate inhibitory and motor centres.

CHEMICAL IRRITATION OF THE NERVES OF THE HEART.—Mosso (*Lo Sperimentale Anno*, XXIV. 1872. Abstract in *Centralblatt*, No. 14, 1873) lends some confirmation to Schiff's old view that the vagus is the probable or only motor nerve of the heart. He chemically irritated the cardiac nerves in dogs poisoned with atropine. In these animals the heart-beats were rendered independent of blood-pressure by the sub-cutaneous injection of atropine, and the nervi vagi recurrentes were irritated by the careful application of a drop of solution of caustic potash. From his experiments he deduces the following conclusions: (1) Irritation of the vagi increases the frequency of the pulse in consequence of the excito-motor fibres running in the trunk of the vagus. (2) If the sheath of the vagi-sympathetic be opened, and the sympathetic be separated from the vagus, chemical irritation of the latter constantly produces an increase of the pulse frequency, whilst excitation of the sympathetic is without perceptible effect on the rhythm of the heart. (3) Mechanical irritation of the inferior laryngeal nerves by simple section is sufficient to increase the pulse frequency, and (4) this quite independent of an increase in the blood-pressure.

ACTION OF THE VAGUS ON THE HEART.—Metschnikoff and Setschenow (*Centralblatt*, No. 11, 1873) from their experiments are led to the opposite conclusion from that of Mosso. When in a turtle (Emys Europea) the irritation of the vagus is carried beyond the bounds, which are known as the phenomena of exhaustion, a very striking picture of the *periodicity* of the inhibitory action of the vagus is obtained. The phenomena were much the same whether the animal had the brain destroyed or not (with intact vagus on the other side), as well as after section of the branch of the sympathetic from the vagus. They draw the two following provisional conclusions: (1) The inhibitory action of the vagus on the heart of the turtle is periodic. (2) In the periodicity of this action is a new proof that the inhibitory fibres of the heart end in a sort of nerve centre. In *Centralblatt* (No. 19, 1873) Setschenow finds also that in the heart of the frog with brain and spinal cord destroyed the action of the vagus is periodic. In frogs the pulse-frequence never rises during the negative phase over the normal. Repechoff investigated the reflex inhibition of the lymph heart by the vagus, and the corresponding phenomena in the blood heart by the sympathetic. In both cases by a continued stimulation of the nerve the effect was certainly periodic.——"Cause of the retardation of the Pulse following artificial or voluntary closure of the nostrils in the Rabbit," by W. Rutherford, in *Journ. of Anat. and Phys.* VII. 283.

ON THE INTERFERENCE OF THE RETARDING AND ACCELERATING NERVES OF THE HEART.—H. P. Bowditch (*Ludwig's Arbeiten*, VII.

259). In a short history of the N. accelerans cordis, whose discovery by the brothers Cyon and A. v. Bezold gave full confirmation to the idea of the latter, upon the necessary connection of the cardiac nerves with the spinal cord, the author gives the results of the researches of M. and E. Cyon upon this nerve in rabbits and dogs, and shows how A. v. Bezold, Beven and Schmiedeberg have increased our knowledge of its function. The latter showed that it is also present in the frog. The branches of the accelerans and those of the vagus are united into one stem before they reach the heart, so that it is possible to irritate both equally strongly, and at the same time. By doing so, we see at first only the inhibitory action of the vagus, and by continuing the irritation there is consequent exhaustion of the vagus, and its action becomes weaker and weaker. After interruption of the irritating stream, the complete action of the accelerating nerve appears. Similar results are obtained when the isolated vagus of the one side, and the N. accelerans of the other, are irritated by currents of the same strength.

It is now necessary to ascertain if both nerves acting oppositely, when these are irritated, yield a medium value of heart-beats, which each would have given if irritated by itself. For this purpose the author used dogs, poisoned with curara, and isolated the N. accelerans by the method of O. Schmiedeberg (*Journal of Anat. and Phys.* Vol. VII. 180). The pulse was written partly with a mercurial and partly with the spring manometer. For a special apparatus for recording the time, we must refer to the original. At first the effect of the maximum irritation of the carefully isolated N. accelerans was studied. No exact proportion could be made out between the form of the curve and the duration, strength, and consecutiveness of the irritation. But probably a nicer gradation of the irritation might yield some results. The latent period varied (from 1 to 22 sec.) in the same and different animals, and was independent of the strength of the induction stream. The only indication of regularity exhibited was that the latent period was shorter when a second irritation followed the previous one before the acceleration of the pulse which it had produced had disappeared. The maximum value which the number of pulse-beats reaches in a given time in the same animal, within narrow limits, unmistakably depends upon the strength of the irritation.

This value, however, is very unequal in different animals. It is a rule without exception, that the time of the duration of the maximum of heart-beats is proportionally short. It is of importance to know how many beats more the heart produces in a given time, when the N. accelerans is irritated, over the number obtained in the same time without irritation. The result is, that the increase of the pulse-beats is greater with the duration of the irritation, but not at all proportionally. In addition to acceleration of the pulse, the irritation of the nerves which are contained in the path of the N. accelerans often produces an increase of the mean arterial pressure. But even where the pressure and the pulse increase at the same time, there is a complete independence the one of the other. Without

exception, after irritation the arterial pressure reaches earlier its maximum than the acceleration of the pulse, and still further, when the pulse-beats have reached their highest value they sink regularly to that which they had before the irritation. The arterial pressure, on the contrary, sinks with wave-like variations under its normal value, and without exception the waves of pressure become lower and shorter the further they are from the beginning of the irritation. The independence of the acceleration-wave, and of the variation in the pressure, are best explained by supposing that there are in the irritated stem two nerves of different functions, one of which acts upon the heart, the other upon blood-vessels elsewhere, in the same way as O. Schmiedeberg has demonstrated that, by irritating a branch proceeding from the ganglion stellatim only, the pressure-wave is produced without at the same time any acceleration of the heart's beats. The author also observes that in some dogs, but not in all, as Fick has already shown, a dicrotic pulse is produced. In those animals in which it was observed it always disappeared when the pulse-beats reached from 210 to 220 in a minute.

By observing the changes of the rhythm of the pulse following irritation of the N. accelerans, the author compares them with those that are obvious in a frog's heart exhausted but warmed, or in the exhausted frog's heart filled with fresh blood serum. The antagonism between the N. accelerans and N. retardans is at least not a complete one. Maximum induction currents, acting at different times upon the isolated accelerans, gave an approximately equal frequence of pulse-beats; the regular tetanisation of the same part of the N. vagus yields not so satisfactory results, e.g. the distance of the coils necessary to produce the slightest perceptible retardation of the pulse must be altered for consecutive irritations. The cause of this irregularity is very probably not to be sought for in exhaustion of the nerve-trunk, but in a changing susceptibility of the automatic apparatus to the irritations of the vagus. It is necessary to find the minimum irritation of the vagus, by which it does not lose its effect on the N. accelerans, to employ this by itself, and then at the same time to employ the maximum of the N. accelerans. From the curves figured at the end of the paper, it is shown that a very weak irritation of the vagus is sufficient completely to overcome the effects of even a maximum irritation of the N. accelerans. From the study of the curves obtained from different animals, it is possible that very weak degrees of irritation of the vagus may not be able to overcome the maximum irritation of the N. accelerans. The vagus has not the power to abolish the condition produced in the heart by the N. accelerans. When the pulse, rendered quicker by the accelerans, was rendered slower by the vagus, the increased pulse-beats returned when the vagus was removed from the induction circuit.

By an increase of arterial pressure, causing irritation of the central ends of the vagus, the author made further investigations to see if this natural irritation would disappear under maximum irritation of the accelerans. For these experiments of course only those animals could be employed whose pulse is retarded by the

increased pressure. The result was, that the irritation of the vagus, effected by increased arterial pressure, was strong enough to overcome a maximum one of the N. accelerans. But, as it has been shown that irritation of the N. accelerans by itself alone can produce an increase of the arterial pressure, so it may happen that by increase of the arterial tension the effects of irritation of the accelerator nerve may be balanced.

So in this manner it can be understood why sometimes in the course of an acceleration, caused by irritation of a very carefully isolated N. accelerans, there appear suddenly some heart-beats with long pauses. In curarised dogs, dying from asphyxia, and in which the blood had already become of a deep dark colour, the author found that the N. accelerans had not lost its irritability but was very active, and it seems to be an important fact for the condition produced in the heart by irritation of the N. accelerans, that oxygenated blood is not necessary for the same.

VARIATIONS OF HÆMOGLOBIN IN THE ZOOLOGICAL SERIES.—Quinquand, *Comptes Rendus*, August 18th, 1873, p. 487. The method consists in determining by aid of a liquid triturated with hyposulphite, the maximum quantity of oxygen, absorbed by the blood, which ought to be effected in five minutes, using 2 cc. of blood (for details, *Comptes Rendus*, LXXVI., 1489, or *Lond. Med. Rec.*, No. 30). (1) The progressive diminution of the quantity of hæmoglobin contained in the same volume of blood follows in general the degrees in the animal scale. At the same time the blood of the primates is not that which contains most. (2) The blood of young animals is less rich in hæmoglobin than that of adults; in many species the placental blood contains as much as the blood of the general circulation. In old age the quantity diminishes. The curve of variations would be represented by a slight fall at first corresponding to the first days of uterine life, the curve then rises in the infant, and remains horizontal during adult life (25 to 50 in man), after which it slowly falls in old age.

ESTIMATION OF THE MINERALS OF BLOOD-SERUM BY DIRECT PRECIPITATION.—L. Gerlach (*Ludwig's Arbeiten*, VII. 99). Dr Pribram's experiments showed that it is practicable to precipitate from fresh blood-serum all the lime and a part of the phosphoric acid which formerly could only be obtained from the ashes of the serum (*Journ. of Anat. and Phys.* VII. 190). Dr Pribram, however, in his experiments took no notice of the magnesia. The author, from his experiments, finds that magnesia as well as lime can be precipitated directly from the serum, but to obtain reliable results it is necessary to precipitate the lime in an acetic acid solution to prevent the contamination of the precipitate by ammoniaco-magnesian phosphate.

WHAT CONSTITUENTS OF ASPHYXIATED BLOOD SERVE TO BIND THE DIFFUSIBLE OXYGEN?—Afonassiew (*Ludwig's Arbeiten*, VII. 71). Alex. Schmidt has shown that asphyxiated blood possesses the property of changing a portion of the oxygen which is added to it, and

that the whole of the oxygen cannot be recovered from blood so treated. In place of an aliquot part of the oxygen, CO_2 is obtained. The author, under Ludwig's direction, wished to know whether the stuff which holds this oxygen is contained in the serum or in the blood-corpuscles. He began with the investigation of the composition of the serum. The serum was obtained free from blood-corpuscles by means of the centrifugal apparatus. He mixed equal volumes of this serum and asphyxiated blood (taken from an asphyxiated animal nearly dead). The quantity of CO_2, oxygen and nitrogen contained in the serum as well as in the blood added, was estimated so that the contents of the mixture in gases could be easily derived therefrom. The gases were pumped out of the mixture and analysed. In five experiments the quantity of CO_2, oxygen and nitrogen obtained was so near the quantity calculated, that it is quite certain that the serum of asphyxiated blood does not contain any substance which could cause the formation of CO_2 from the oxygen of the blood-discs. In one experiment the quantity of CO_2 actually obtained was 32·23 per cent., while the quantity reckoned was 32·40 per cent., and the actual quantity of oxygen obtained 7·24 per cent., the calculated amount 7·31 per cent.; small variations quite within the limits of analytical errors. The author now added a given quantity of oxygen to a known quantity of asphyxiated blood whose gaseous contents were known, and then ascertained the amount of gases in the same. Thus in one of the experiments the asphyxiated blood contained 1·48 volumes per cent. of oxygen, 11·12 per cent. of oxygen was added, 11·86 per cent. of oxygen was obtained. So that 0·74 volumes per cent. of oxygen had disappeared and 0·37 per cent. of CO_2 more was obtained. The serum of the same blood, on the contrary, absorbed only 0·21 volumes per cent. of oxygen and yielded 0·16 volumes per cent. more of CO_2. In a second experiment, after the addition of oxygen, 1·04 volumes of oxygen disappeared from the asphyxiated blood and oxygen, ·93 volumes CO_2 more was obtained. From this it appears that the substance in asphyxiated blood, which allows the finding of the oxygen and the formation of CO_2, is present in the floating elements of the blood (red and white corpuscles).

Expulsion of Nitric Oxide from the Blood.—Zmitz and Donders have shown that CO can be expelled by other gases, e.g. oxygen, from its hæmoglobin compounds. Podolinski (*Pflüg. Archiv*, VI. 1872, 553) has also succeeded in driving out completely CO from blood containing it, by means of oxygen (atmospheric air), and also by hydrogen, though hydrogen acts a little slower. Nitric oxide can also be expelled by hydrogen, though here also longer time is required than for the CO.

Estimation of the Absolute Quantity of Blood.—Steinberg (*Pflüg. Archiv*, 1872, VII. 101) in his researches has adopted the method of Preyer, viz. the giving of a green light in the spectrum by a certain estimated solution of hæmoglobin, which serves as a test object for the washings. The proportion of blood to the body

weight in the rabbit was 1 : 12·3 to 13·3. In guinea-pig, 1 : 12 to 12·3. Adult dog, 1 : 11·2 to 12·5. Young dog, 1 : 16·2 to 17·8. Adult cat, 1 : 10·4 to 11·9. Young cat, 1 : 17·3 to 18·4. The colorimetric method of Welcker adopted by Bischoff, Heidenhain, Ranke, &c. yields very similar results.

Minute Moving Particles as Constant Constituents of Normal Blood.—Nedsvetski (*Centralblatt*, 1873, No. 10) finds that in perfectly fresh human blood, examined with a power of not less than 900 to 1000 diams., there is to be observed in addition to the red and white corpuscles very small spherical bodies without cilia of the size of the granules of the white blood-corpuscles, which exhibit movements of rotation round their axis, and move from place to place. No structure is to be observed in them. Nedsvetski has named these bodies harmo-cocci. They attach themselves to the filaments of fibrin when it is found under the microscope. Other bodies, whose source seems to be the white blood-corpuscles, are also described by this author. Still other granules are to be distinguished; they resemble the white blood-corpuscles, and are to be distinguished from them by being more rounded, and by being devoid of movement. They remain longer without change than the other elements of the blood.

Bacteria-forming Masses present in Blood.—Osler and Schäfer (*Centralblatt*, 1873, No. 37). In many diseases there is present in the blood a greater or lesser number of colourless granular bodies, of the same size, or often larger than the white blood-corpuscles, and consisting of small pale particles. When a drop of blood is diluted with solution of Na Cl ($\frac{3}{4}$ per cent.) and kept at the temperature of the body, small fibres are observed to be pushed out from the surface of these masses which soon exhibit a violent vibratile movement, while at the last they separate themselves from the mass and float free in the fluid. Every fibre exhibits either in its middle or at one of its ends a swelling, which, according to the position of the fibre, appears either circular or linear. The swelling is not globular, but discoid. These masses are present sometimes in apparently normal human blood as well as in that of animals, but they do not exist previously as such in the blood-vessels. They are formed after extraction of the blood by the flowing together of the pale particles from which they are made up.

Action of Quinine on Blood.—Binz (*Arch. f. Expt. Path.* i.) pointed out some years ago that Quinine arrested the movements of the white blood-corpuscles, and this he explains by the drug diminishing the oxydizing power of the red blood-corpuscles, for the white corpuscles are only active when supplied with oxygen, which is yielded up to them by the red corpuscles as they pass. Binz has further found that when oxygen is withheld from them they do not penetrate the walls of the blood-vessels, and his observation has been confirmed by Heller and Zahn.

Colouring Matters of the Blood.—H. Struve (*Virchow's Archiv*, lvi. 423) has succeeded in discovering two colouring mat-

ters in the blood. The one, probably identical with Virchow's hæmatoidin, is crystalline, dark blue in colour, insoluble in water, alcohol, ether, chloroform, and acids, but dissolving in weak alkalies, giving the solution a brownish tinge. On heating it, ammoniacal fumes are evolved. The other is soluble in water and in alcohol, but with difficulty in ether, and is probably identical with Von Wittich's hæmatin.

On the Sugar-producing Ferment of Blood.—P. Plósz and E. Tiegel (*Pflüg. Archiv*, 1873, VII. 391). When defibrinated blood is mixed with a weak solution of Na Cl, and the blood-corpuscles allowed to separate, the supernatant fluid contains the ferment in large quantity, while the blood-corpuscles contain none, or only a very small quantity. The authors conclude that the Na Cl solution has extracted the ferment from the blood-corpuscles, and quote the analogous phenomenon that it is possible by a 3 per cent. solution of Na Cl to extract from fibrin a sugar-producing ferment nearly allied to globulin. The authors, in repeating Bock-Hoffman's experiments, found in the excreted urine a sugar-producing ferment; also in diabetic urine a substance which, when freed from sugar, possessed the same property. The authors' experiments render it probable that in the Bock-Hoffman experiments the formation of sugar takes place not in the kidneys, but in the blood or liver. With regard to V. Wittich's experiments, by which he sought to prove the existence of a special liver-ferment which changes glycogen into sugar, V. Wittich has stated (*Pflüg. Archiv*, 1873, 28, abstract in *Lond. Med. Rec.* 1873) that in a liver quite free from sugar and from which the blood has been completely washed out, after some time the sugar returns, and the sugar-producing ferment can be demonstrated. The authors think it more probable, however, that by the washing out of the liver with water, the ferment contained in the blood was dissolved, and fixed in the coagulated liver-cells.

"Presence of Soluble Earths and Phosphoric Acid in Alkaline Blood," Fokker, in *Pflüg. Arch.*, 1873, VII. 274. "Absorption of Oxygen by Blood," Gréhaut, in *Comptes Rendus*, LXXV. 495, abstract in *Lond. Med. Rec.*, No. 17. "Physical Nature of the Coagulation of the Blood," A. H. Smee, in *Journ. of Anat. and Phys.* VII. 210. "Observations on Welcker's Method of Estimating the Quantity of Blood," Gscheidlen, in *Pflüg. Arch.* VII. 530.

Lymph.

Secretion of Lymph in the Fore-limbs of the Dog.—(*Ludwig's Arbeiten*, VII. 198.) Paschutin has selected by preference the fore-limbs of the dog as the source of the lymph, because the roots of the brachial-lymph stem arise only in the skin and muscles, and the lymph is not mixed with that of other tissues, as would be the case if it were obtained from the truncus thoracicus, and, further, because the circulation in the limb is easily modified, and its nerves excited. On the living animal, after cutting the skin at the outer margin of

the vena jugularis, all small arteries must be tied, and all blood removed. The A. transversa colli is next sought for by aid of a blunt instrument, and two ligatures are placed upon it, and the artery cut between them. Immediately in the neighbourhood of this vessel, but nearer the spinal column, the lymph stem is to be found. By pressing the lymph from the limb upwards towards the head, the trunk is easily rendered visible. Place a ligature on the vessel as near as possible to its place of opening into the truncus colli; but before tying the ligature see that no small branches join the vessel from the scapular or pectoral regions; if so these must be ligatured. The brachial stem must be carefully isolated for 5 mm., a ligature placed round it, and a canula bound in it. The best material for making the canula is glass. The observations of Generisch and Lesser have shown that the lymph only flows regularly from the limbs, when these are actively or passively moved; and that further, the rapidity of the out-flow probably depends upon the intensity of the movements and the tact displayed. It was therefore necessary to have the limb moved regularly, and the method by which this was obtained, by means of a machine, is to be found in the original. In order to vary the experiments it was necessary to divide the plexus brachialis, the cervical spinal cord, or both. The plexus brachialis is easily reached through the original wound in the skin. In dividing the spinal cord in the neck, the best spot is at the lower margin of the second cervical vertebra; but the loss of a small quantity of blood causes the death of the animal. It is therefore better, in laying bare the vertebra, to ligature the large branch of the A. profunda cervicis, which runs backwards on the middle layer of the cervical muscles. After section of the spinal cord artificial respiration is kept up, and the electrodes for irritation of the spinal cord applied. The blood-pressure was measured in the A. carotis.

I. *The medium rapidity of out-flow of Lymph.*

1. All experiments show that with the duration of the experiment the out-flow of lymph is diminished, i. e. when the circumstances, under which the animal is operated on, remain the same. But it is different whether the animal is poisoned or not, or placed under the ordinary temperature of the air or if the temperature is higher, or whether the spinal cord is injured or remains intact. With regard to the source of the lymph it is easy to show that one has not to do merely with an emptying of the lymph vessels, for when the lymph is carefully pressed out of the limb with the hand, and this is again repeated immediately thereafter, employing very powerful pressure, no greater quantity will be obtained. The assumption that during the time of the experiment a store of tissue juice is expelled, the author cannot refute, considering the striking proofs which Hammarsten has had, on account of the large quantity of lymph which he obtained. But not less important is the observation that in one experiment, after the weak movements of the limb caused by the machine had been followed by the powerful one caused by the hand, the rapidity of out-flow rose suddenly, and lasted 25 minutes, without

however, sinking to the small quantity which was obtained before the commencement of the powerful pumping. From this, and specially from the composition of the lymph, every portion of which has a different value, it seems to the author very probable that the outflowing lymph is actually secreted during the period of movement. And again, in several experiments the out-flow of lymph during the period of rest either ceased entirely, or only a very small quantity could be obtained on pressing the limbs; but as the pumping movement again increased, no more lymph flowed than was dropped by the same movement before the rest. It is to be observed that these results were obtained also on animals whose cervical spinal cord and plexus brachialis were cut. The observation that the secretion of lymph ceases during the period of rest is not valid for all cases.

2. *Action of Curara on the Secretion of Lymph.*—The experiments of Lesser rendered it very probable that curara modified the secretion of lymph. The cervical spinal cord and plexus brachialis were cut, and then the lymph pumped out for a certain time; the animal was then rapidly poisoned with a sufficient quantity of curara, and the lymph obtained as before. After curara poisoning the rapidity of secretion increases, reaches its maximum in 40 to 50 minutes afterwards, and then by degrees gradually decreases. This acceleration takes place not only during the period of movement, but also during rest. The increase and decrease of the secretion does not run parallel with the arterial blood-pressure. Curara produces redness of the skin. It also modifies the composition of the secreted lymph.

3. *Change of Lymph Secretion in consequence of an increased supply of Blood.*—The simplest method to produce arterial congestion in the fore-limb of a curarised dog is to lay bare carefully the brachial plexus, then pump out the lymph under these conditions, and then, after the section of the plexus, to proceed with gaining the lymph. After this the foot became warmer, and blood flowed from the foot, after puncture with a needle, much more freely than before. Tables of the results so obtained show very clearly that increased supply of blood is without influence on the secretion of lymph, that its rapidity fell regularly after section of the nerves, notwithstanding that the blood-pressure after the same was higher than before, and notwithstanding that the foot and upper part of the extremity was more richly supplied with blood. To be without doubt on this point, the cervical spinal cord was cut and electrodes applied, and at the same time the plexus brachialis was divided. One could therefore change, after irritation of the spinal cord, the lymph caught during high and during low blood-pressure, and therefore be sure that the change of the blood-pressure measured in the carotid also took place in the fore extremity, because the vaso-motor nerves of the same were paralysed. From numerous results it is impossible to doubt that in the dog increased blood-supply is without influence on the secretion of lymph. At no time was there produced by the appointed irritation even a stoppage in the sinking of the rapidity of secretion, to say nothing of an increase of the latter. These results, therefore, stand in open contradiction to the idea of the dependence

between the blood-pressure and the lymph secretion. Till now it was believed that the cause of the movement of fluid from the blood-vessels in the tissue spaces was founded in the difference of pressure which was active on both sides of the wall of the vessels.

4. *Change of the Secretion of Lymph by increased temperature of the body.*—To test whether the cooling which the animal undergoes when it has lain for a long time on the operating table is the cause of the secretion becoming smaller and smaller with the duration of the time of observation, the animal was allowed to cool at the ordinary temperature while the lymph was obtained, and whilst the lymph was being obtained the animal was warmed from without. For this purpose the double walled box (described at p. 203) was employed. An increased temperature accelerated the out-flow of the lymph.

II. *Per-centage composition of Lymph Serum in fixed residue.*

1. To free the lymph serum from fibrin and corpuscles the lymph, which during the catching was shaken before evaporation, was placed in closed vessels in the centrifugal machine, until the clear serum could be poured off from the sediment. A weighed quantity of the latter was carefully dried. In 84 different examples of lymph, taken from different dogs or from the same dog at different times, the percentage composition of lymph serum in fixed residue clearly changed. The variation was from 2·61 to 6·55 per cent. From numerous experiments it is shown that the per-centage increases with increase in the time of experiment. Similar results were obtained by Generisch when he collected the lymph from surviving limbs whilst an artificial blood stream was passed through the same. The formation of the lymph is not due merely to transudation.

2. In the course of an experiment when the initial slowing of the rapidity of the lymph-secretion again increases, the per-centage of its serum very often decreases again, and this independent of the cause which produces the acceleration. The relation which exists between the lymph which flows out in a certain time, and its per-centage in fixed constituents, is here very obvious. For when the rapidity of secretion was accelerated by new means, in spite of the continuation of these means, the rapidity becomes by degrees less and less, the per-centage of the residue increases.

3. In one experiment, in the course of which the animal was poisoned with curara, the contents in fixed constituents rose 1·2 per cent. It is therefore to be expected that all the animals narcotised with this poison before yielded a concentrated lymph. When in seven cases the animals were curarised before the beginning of the experiment, the per-centage of the first portion of lymph varied from 4·4 and 6·5 per cent., i. e. it had already reached in the beginning of the experiment the worth which is only reached towards the end in unpoisoned animals. In short, when in the curarised animals, in consequence of a cause, the rapidity of out-flow of the lymph increases, exactly the same proportions in the per-centage of the lymph in fixed constituents occur as in the non-curarised animals, but not

with the same regularity. In other experiments the rapidity of out-flow of lymph was increased by warming the animal. In these experiments the contents of the lymph in fixed stuffs decreased, although the rapidity of secretion had already begun to decrease. If these results are confirmed, there would be proved a specific action of increased temperature on the composition of lymph serum which was secreted in animals with uninjured spinal cord. With regard to the question whether the lymph is obtained by the passive movements of the limbs, or is formed from a previously prepared store during the period of the experiment, the different composition of the obtained fluid speaks in favour of the latter. And further, this remains scarcely doubtful when, the movement remaining the same, the composition of the serum changes after this more or less rich out-flow. It further shows how insufficient it is to observe solely the volume of the fluid, for its composition must also be investigated.

It is not possible to explain the results obtained simply by the categories of a propelling force and a resistance, for two conditions which equally increase the out-flowing volume act quite in an exactly opposite manner on the quantity of albumen of the serum, as is the case with curara in opposition to the active movements of the limbs and the warming of an injured animal.

Respiratory System.

Quinquand, "Respiration of Fishes," in *Lond. Med. Rec.* No. 22.——Moin, "On Cubic Space and Volume of Air," in *Comptes Rendus*, Aug., 1873. Abstract in *Lond. Med. Rec.* No. 33.——Schroetter, "Movements of the Trachea." Abstract in *Lond. Med. Rec.* No. 27.——"Graphic Representation of the Respiratory Movements," *Deutsch. Archiv f. Klin. Med.*, 1872, x. 124, and 1873, xI. 379.——"Erectile Action of the Blood-Pressure in Inspiration," Hoggan, in *Edinb. Med. Jour.*, 1872, CCVIII.——"Functions of the Nerves and Muscles of the Larynx," Schech, in *Zeit. f. Biolog.*, IX. Hft 2, 258.——"Movements of Respiration," E. Lockenberg, in *Arb. aus d. Phys. Lab. d. Würzburger Hochschule*, Pt. II. 199.——"Dyspnœa from Warmth," Goldstein, in *ibid.* Pt. I. 77.——"On Respiration," Nussbaum, in *Pflüger's Archiv*, VII. 296.

Action of Intercostal Muscles.—From experiments on a guillotined criminal, Onimus (*Gaz. Hebr. de Méd. et de Chir.*, Jan. 24, 1873) has shown by means of electricity, that the external intercostals raise the ribs and are inspirators, while the internal ones depress them and are expirators, thus confirming Hamberger's theory: muscular contractility was further observed not to disappear in all the muscles at once. The diaphragm and muscles of the tongue are the first to lose their excitability, while the masseters retain theirs longer than any muscle of the face. (Abstract in *Lond. Med. Rec.*, No. 9.)

Influence of Artificial Respiration in Strychnia Poisoning. Rossbach, *Centralblatt*, 1873, No. 24. The author concludes from

his experiments on rabbits, that artificial respiration has no influence either upon the preservation of the life of the strychnia-poisoned animal or upon the intensity or duration of the spasms.

Apnœa.—Ewald (*Pflüger's Archiv*, vii. 575) used always the same animals for comparing the oxygen contained in the blood. A quantity of blood was obtained during ordinary respiration, and its gases obtained. Apnœa was then produced by inflation, another quantity of blood was obtained when the animal had completely recovered, and so forth, and the quantity of oxygen was estimated at the different periods. The chief results of these experiments are in the condition of apnœa; the oxygen of the artificial blood is increased almost to complete saturation, that of the venous blood is diminished, while the CO_2 was clearly diminished in both cases. If the animal again breathed naturally, the quantity of oxygen of the arterial blood sank to the normal, the quantity of CO_2 also increased. By natural forced respiration almost complete saturation of the blood by oxygen can be produced.

That the diminution of oxygen in venous blood is not produced by a diminution of temperature through the blood-lettings, whereby the tissues would have increased need for oxygen, is shown in the experiments where the animals were kept at a constant temperature. Withdrawal of air during apnœa causes asphyxiation much later than during natural respiration. In apnœa, after cessation of artificial respiration, after a long time (40 seconds), at first a scarcely perceptible diminution of oxygen appeared, which, as well as the mentioned retardation of asphyxiation, speaks for a diminution of the consumption of oxygen. Experiments of Pflüger, not yet published, however, speak against this; he observes neither increase nor diminution in the consumption of oxygen in apnœa. The author would explain the fact mentioned through a diminution of the rapidity of the blood stream, and he found that the blood-pressure during apnœa was diminished about one-third of the normal. During natural respiration the oxygen contained in arterial blood was about 5·5 per cent. higher than that of venous, during apnœa about 7·1 per cent.

Alimentation.

"Physiological Studies on the Action of Flesh-broth, Extract of Meat, Alkaline Salts, and Kreatin." Bogoslowsky in *Arch. f. Anat. u. Phys.* 1872, 347.——Seegen and Nowak, "Estimation of Nitrogen in Albumates," *Pflüg. Archiv*, vii. 1873, 284.——Pettenkofer and Voit, "Feeding with Flesh and Fat," *Zeitschr. f. Biolog.* ix. 1873, 1.——Nasse, "Studies on Albumen," *Pflüg. Archiv*, vi. and vii. 589.——Wolffhügel, "Digestion of Fibrin without Pepsin," in *Pflüg. Archiv*, vii. 188.——Sanson, "On a Mechanical Coefficient of Food," in *Comptes Rendus*, No. 24, 1490—1873. Abstract in *Lond. Med. Rec.* No. 30.——Mohlenfield, "On Peptones," *Pflüg. Archiv*, Vol. v. Abstract in *Lond. Med. Rec.* No. 27.——Von Wittich, "Action of Pepsin on Fibrin," in *ibid.*——"Contribution to the Physiology of

Water," Falck in *Zeitschr. f. Biolog.* VIII. 398.——"Absorption of Fat," Radziejewski in *Virch. Archiv,* 1872, LVI.——"On the importance of Chloride of Sodium and its relation to Potassium Salts in the human economy," G. Bunge, *Zeitschr. f. Biolog.* IX. Hft 1, 104.

ASSIMILATION OF FATS.—Hoffman, *Zeitschr. f. Biolog.* VIII. Abstract in *Lond. Med. Rec.* No. 17. The animals were starved till all the fat was supposed to have disappeared, when they were fed on nearly pure fat, in order to determine whether fat is deposited in the tissues from the food or not, or whether it first undergoes conversion. On post-mortem examination it was found that the deposit is chiefly in the liver and mesentery. Upon analysis it was shown that a considerable quantity was assimilated and deposited in the tissues.

BRUNNER'S GLANDS.—Krolow (*Inaug. Dissert.* Berlin, 1872) finds that the fluid secreted by these glands has the power of converting starch into sugar, and completely dissolves raw fibrin, but has no effect upon fat and coagulated albumen.

RESEARCHES ON THE FUNCTION OF THE GLANDS OF THE INTESTINAL MUCOUS MEMBRANE.—Costa (abstract in *Centralblatt,* No. 20, 1873). The experiments were made upon horses, which were killed during digestion. The glands of Brunner and Lieberkühn were specially prepared after Von Wittich's method (extracted by glycerine), and with the fluid so obtained digestion experiments were made. His results are: (1) That the glands of Brunner possess strongly the power of converting starch into sugar, but have no effect on albumen or fat; thus confirming Krolow's observations. (2) Extract of Lieberkühn's glands of the small intestine immediately changes starch into sugar, but is also without effect on the albumen and fat. (3) The extract of Lieberkühn's glands of the great intestine has no diastatic effect, and is neutral with regard to albumen and fat. (4) The extract of Brunner's is in the horse, and especially in the dog, very thick and stringy, and on the addition of acetic acid deposits flakes of mucus. (5) The extract of Lieberkühn's glands is less thick and more fluid, but besides its diastatic function it also serves to keep the contents of the small intestine fluid. (6) The intestinal juice, the mixture of the separately observed secretions, has no other effect than its constituents.

THE NUTRITIVE VALUE OF PEAS AND FLESH, AND THE QUANTITATIVE RELATION BETWEEN THE NITROGEN INGESTED AND THAT EXCRETED IN THE URINE.—Woroschiloff in *Berlin. Klin. Wochensch.* 1873, No. 8. Abstract in *Lond. Med. Rec.* No. 18. The experiments were made upon himself. Both peas and flesh yielded complete nourishment when the requisite quantity of carbo-hydrates in the form of bread and sugar and a small quantity of Na Cl were added. The power of assimilation for the albuminous constituents of flesh is higher than for those of peas, for the amount of nitrogen in the fæces in the first case was only 6—10 per cent. of the ingested nitrogen, in the latter 10—17 per cent. During violent muscular exercise (lifting

a heavy weight) the quantity of flesh and peas must be increased to keep the body-weight unchanged. The quantity of urea excreted under these circumstances was not increased. A quantity of the assimilated nitrogen was retained in the body to be employed for repairing the muscle substance. From this and other observations it seems probable that the source of muscular power is to be sought for in the non-nitrogenous constituents of muscle.

ON THE FERMENTATIVE ACTION OF PANCREATIC JUICE AND PAROTIDEAN SALIVA OF NEWLY-BORN CHILDREN AND INFANTS UPON STARCH.—Korowin, *Centralblatt*, No. 17, 1873. The pancreas was taken from children which had died of various affections, chiefly intestinal and pulmonary, and at different times after death. In all cases where possible the glucose was estimated quantitatively. The results obtained are: Pancreatic juice in the first month showed no effect in changing starch into sugar. In the second month it begins to exert the fermentative change, and at the end of the third month the action was so strong that in several cases the quantity of sugar could be estimated quantitatively. The complete effect is fully established at the end of the first year. The parotid secretion converts starch into sugar in the first day of life, and the action is so strong that the quantity can be easily ascertained. In *Centralblatt*, No. 20, 1873, Korowin details his method of obtaining the saliva. The children are allowed to suck small pieces of meerschaum, from which the saliva is pressed out. It is possible to collect the saliva in the first minutes after birth. The results are corroborated by Schiffer (*Arch. f. Anat. u. Phys.* 1872, 464.——"Effects of Stimuli on the secretion of the Parotid Gland," by P. B. Stoney, in *Journ. Anat. and Phys.* VII. 161.

PLACE OF DESTRUCTION OF ALBUMEN—THE ANIMAL ORGANISM.—Hoppe-Seyler, *Pflüg. Archiv*, VII. 1873, 399. This paper is of a critical nature, and is partly directed against the idea of an organic and circulating albumen, introduced into physiology by Voit. Lehmann, Frerichs, and then Bidder and Schmidt, believed that from the great rapidity with which increased nitrogenous diet was followed by an increased excretion of urea, that this urea was produced directly from the albumen of the food without the food having previously become a constituent of the tissues of the body. Liebig imagined that occasionally a part of the nitrogenous material employed as nutriment might form urea without having previously been a constituent of the body. Then Voit from his observations concluded that there was an organic and a circulating albumen. According to his view only the albumen taken in as food, and which is flowing in the organs, is disposed to be broken up, while, on the contrary, the albumen which composes the organs of the body is so only when it is again rendered fluid and again returned to the general juice-stream. This view, however, is in opposition to a great number of facts. Shortly, the deductions of the author are that the blood and the lymph vessels possess neither a ferment nor the peculiarities for oxydation which would lead one to believe that the place of breaking-up of the nutriment is to be

sought for either in the blood or lymph; on the contrary, we know chemical changes in the composition of the glands and muscles, which show that the albuminous matter of the organ can be decomposed comparatively quickly by fermentation and oxydation. The idea that urea is formed in the blood directly from the surplus nutriment is untenable, but much more is the idea of Voit of an organic and circulating albumen to be rejected.

Stomach ferment of Cold-blooded Animals.—A. Fick, *Verh. d. Würzb. Phys.* 1873, iv. 120. The lowest temperature at which the stomach ferment of the mammalia is active was fixed by Schiff at + 13°, and by Kühne at + 5°. If digestion in the cold-blooded animals does not cease when the temperature sinks to 5°, then in this case we have to do with another ferment. The watery acid extract (HCl) of the mucous membrane of the stomach of the dog and pig exhibits digestive powers even under 10°, but at 0° never, while, on the contrary, a similar extract prepared from the frog, trout, and pike digests fibrin at 0°, and at 40° was quite as active as that of the dog. The stomach ferment of cold-blooded animals is therefore not quite identical with that of the warm-blooded.

Formation of Pepsin in the Stomach.—Ebstein and Grützner, *Pflüg. Archiv,* viii. 122. This paper is a reply to that of Von Wittich (*Pflüg. Archiv,* vii. 19), in which he states that the pyloric glands have no pectic action. They find that an extract of the pyloric made with HCl possesses digestive action, but an extract of the same with glycerine does not possess this property, as Von Wittich found. They find that HCl alone does not dissolve so much albuminate as HCl with only the smallest trace of pepsin added to it. They conclude that the pyloric glands do possess digestive powers, and that they do not obtain their pepsin by infiltration from the fundus; and further, from their experiments, they think it in the highest degree probable that the chief-cells ("Hauptzellen") of the fundus and the glandular cells of the pylorus prepare a secretion, in which pepsin is present not yet free, or not completely developed. It is first freed or completely developed by contact with Na Cl, or HCl (or probably, in general, with all combinations of chlorine).

Metamorphosis of Food and Tissue in Flesh and Fat Diet.—Pettenkofer and Voit (*Zeitschrift f. Biolog.* ix. Heft 1), experimenting on their famous dog, determined what becomes of fat when taken together with flesh. They give tables which show the effects of dieting with various proportions of fat and flesh. They find that fat is taken up in large quantities from the intestine. In one experiment, lasting over fifty days, the animal was fed with 500 grms. of flesh and 200 of fat; 14·7 grms. of dry fæces were evacuated, containing 4·6 of fat, so that in every 24 hours 195·4 grms. of fat out of the 200 were absorbed. Although when a moderate amount of fat is given some part of it is discharged by the fæces, yet this is not because it cannot be absorbed, for if larger quantities are given nearly all is taken up. Fat it seems splits up in the animal economy

into simpler compounds with greater difficulty than albumen; indeed fat resulting from the decomposition of albumen is more easily oxydized than that in the food.

PHYSIOLOGY OF THE MOVEMENTS OF THE INTESTINES.—Horvath, *Centralblatt*, Nos. 38, 39, 40, 41, and 42. In 1869 the author showed that in artificially cooled animals the intestines became completely motionless at a certain degree of cooling, and did not contract when electrically stimulated, but that upon warming the same piece of intestine resumed its normal movements and its sensibility to electrical irritation. When a piece of excised is changed from cold to warm water its movements decrease very obviously with the time, so that at the 3rd or 4th change from the cold to the warm water the movements are scarcely perceptible. Excised living intestine when placed in warm water always everts its edges, while the edges of dead intestine never show such an eversion. The abdomen of an animal was opened, and through a loop of its intestine well supplied with blood, and in connection with the general circulation, by means of the mesentery, water of different degrees of temperature was passed. For irritating it a Du Bois Reymond's induction machine and one or two Daniell's elements were employed. By the passage of cold water the loop of intestine remained motionless (10 to 15 mins. at most), till the cold was compensated by the heat, which produced the peristaltic movement. At all temperatures between + 19° C and 0° C the intestines remained motionless and insensible, and that for a long time; but between + 19° C and + 41° C, above which no experiments were made, they exhibited spontaneous movements, and contracted when stimulated electrically. The peristaltic movements (between + 19° C and + 41° C) rose somewhat proportionately with the increase of temperature. Similar results were obtained from the great intestine, cœcum, and different parts of the small intestine in cats, dogs, guinea-pigs, and rabbits, and in the stomach of the frog. A pale-coloured piece of intestine, i.e. with little blood, in spite of the warming, contracts less energetically than a piece well supplied with blood. Heat is the only till now known means of producing peristalsis in a piece of intestine rendered for a long time motionless by cold. On warming a piece of cooled intestine a second before the movements begin it suddenly widens. For the movements of the intestines in addition to heat an ample supply of blood is necessary, without which the peristalsis is very weak, or ceases altogether. The author thinks that the diarrhœa of abdominal typhus is perhaps alone to be ascribed to the elevated temperature and the thereby increased peristalsis. The diarrhœa and constipation in different affections may be explained by a combination of the temperature and the addition of blood to the intestines. The intestines may remain motionless for weeks or months during hybernation, and they doubtless play an important part in hybernating animals. From experiments upon artificially cooled animals the author is of opinion that there is a connection between the death of such a cooled animal and the absence of motion in its intestines.——"Peristaltic movements

of the Stomach and Intestines." Van Braam Honckgeest, *Pflüg. Archiv*, VI. 266. (Abstract in *Centralblatt*, No. 29, 1873, and VIII. 163.)

Liver.

"Cholic Acid." Baumstark in *Berlin Klin. Wochensch.* 1873, No. 4.——"Source of Glycogen in the Liver." Weiss in *Sitz.-ber. d. Wien. Acad. d. Wiss.* LXVII.——"On the Albuminous Substances of the Liver-cells," in *Pflüg. Archiv*, 1873, VII. 371.——"Absorption Spectrum of Hydrobilirubin." C. Vierordt in *Zeit. f. Biol.* IX. 160. ——"Wistinghausen's Endosmotic Experiment on the Action of the Bile in the Absorption of Neutral Fat." Steiner in *Reich. and Du Bois Rey. Arch.* 1873, 137.——"Formation of Colouring Matter of Bile." Steiner in *ibid.* 160.——"Lectures on Diabetes and the Glycogenic Function of the Liver" (Translation of, from *Revue des Cours Scientifiques*), *Lond. Med. Rec.* No. 40, 1873.

SECRETION OF BILE.—Röhrig in *Medicin. Jahrbücher*, Heft II. 1873. A bent glass tube was introduced into the duct. communis choledochus of a curarised rabbit, in which artificial respiration was kept up. At the outset of the experiment one drop of bile was obtained every 8 secs., but towards its close one drop in 17 secs., when the bile was more glutinous and deeply coloured. Compression of either the vena portæ or the hepatic artery or both together caused a diminution of the quantity of bile, though after a time the liver recovered to some extent. In the dog ligature of the thoracic aorta above the diaphragm diminished the flow of bile largely from 1 drop in 7 to 1 in 50 secs. Ligature below the cœliac anis in the rabbit raised it from 1 in 8 to 1 in 3. Injection of water into the intestine and digestion always increased it. Section of the splanchnics increases it. Division of the spinal cord is followed at first by increase and then by diminution of the flow. Compare also I. Munk, "On the Excretion of Bile by irritation of Sensory Nerves." *Pflüg. Archiv*, VIII. 151.

ACTION OF THE BILE IN CONVERTING STARCH INTO SUGAR.—*Pflüg. Archiv*, VI. Von Wittich states in opposition to Ranke that absolutely fresh bile has the power of converting starch into sugar. Also *Pflüg. Archiv*, VII. Heft 1, 1873. Von Wittich from his experiments thinks that the liver, even when washed free from blood, contains a ferment capable of converting starch into sugar, and that there is good evidence to show that this ferment is generated in the liver-cells.

Milk.

"Organic particles in Milk." Béchamp in *Comptes Rendus*, 1873, x. 654.——"Cause of Coagulation of Caseine by Rennet," in *Journ. f. Pract. Chem.* 1872, VI. 174.

COMPOSITION OF HUMAN MILK.—Th. Brunner, *Pflüg. Archiv*, VII. 440. The author undertook this investigation after seeing an

analysis by Sourdat (*Comptes Rendus*, LXXI. 87, 1870), in which the milk from the right breast differed in its composition from that of the left. The author estimated the albuminous substances together. These substances (casein and albumen) are easily and completely separated on adding dilute acetic acid till the alkaline reaction disappears. The precipitate when washed, and so freed from any sulphate of soda that may be in it, was then dried and weighed, and represents the casein + albumen + fat. The fat was then estimated in another portion of milk by Trommer's method. By subtraction the weight of the casein and albumen was easily obtained. The sugar was determined by suturation with Fehling's solution in the usual way. From a table containing 14 double analyses (milk from right and left breasts), with remarks upon the age of the woman, and number of children, the author gives the following as the mean composition of the collective analyses in 100 pts.: 0·63 albuminous bodies (casein and albumen), 1·73 fat, 6·23 sugar, 90·00 water, and 1·41 soluble salts and extractives. The quantity of albumen in these analyses is far under that cited by Vernois and Becquerel, viz. 3·92. The author thinks that this is to be accounted for in that these authors did not estimate the albumen directly, but all the other constituents, and regarded the remainder as casein, and that they did not completely dry the milk. The author further remarks that, while other experimenters have analysed milk soon after delivery, his analyses were made on milk obtained several months after delivery, when it seems that the milk becomes with the time poorer in albumen and fat, whilst the other constituents remain tolerably unchanged. A table is then given showing the composition at different periods after delivery. The milk of the cow is poorer in water, sugar, soluble salts, and extractives, than human milk, but much richer in albuminous stuff and fat. From a table, indicating the composition of the milk taken from the right and left breasts at the same time, it is shown that the milk in each can have a different composition.

Muscle.

ON THE FATIGUE AND RECOVERY OF TRANSVERSELY STRIPED MUSCLES.—H. Kronecker, *Ludwig's Arbeiten*, VI. 177. This communication is specially interesting in that the results from a very large number of carefully conducted experiments, seem to stand in opposition to the generally accepted view of the influence of work upon the consumption of material. According to the laws of the change of material one would expect that excised muscles, which have only a distinct quantity of material at their disposal, would consume this by a corresponding quantity of work; thus, by a single great effort quickly, by a small one slowly. Heidenhain (*Mechanische Leistung, Wärmeentwicklung und Stoffumsatz bei der Muskelthätigkeit*, 1864) has found that the warming of a muscle is always greater during its contraction the greater the weight it lifts, and also that with the principal mechanical the secondary thermal expenditure increases.

One has therefore more ground for the belief that a muscle when it raises a large weight must exhaust its force much sooner than when it lifts none. The author has found that the fatigue is quite independent of the work, and follows very simple laws, and really depends upon the frequency with which the single acts follow each other.

To estimate exactly by comparison the dependence of the fatigue on the period of rest, on the weight, on the strength of the irritation, and also the influence of substances circulating in the muscles, the author wished to irritate the muscles (triceps femoris) of a frog loaded or overloaded ("belastet oder überlastet") with selected weights, by opening and closing induction-strokes of selected intensity, in opposite directions and in easily changed intervals of time, and to have the height of the contractions written on paper in equal small (1 mm.) distances. The following arrangement of apparatus was employed.

The two corresponding muscles (triceps femoris) of a frog were placed by means of a string in connection with two levers for writing, which marked the height of the contractions (enlarged twice) upon the paper on the cylinder of a large kymographion. For irritating, opening, or shutting, induction-strokes, which affected directly both muscles (after the one pole was placed to the lower end of one muscle and the other pole to the lower end of the other), were employed. The irritation was increased till it produced maximum contractions before the proper experiment commenced. By means of a metronome, which closed the primary circuit of a du Bois Reymond's induction-apparatus, induction-strokes were discharged in equal intervals of time (whose length could be easily changed within wide limits), one sort of the induction-strokes (opening or shutting stroke) could be rendered ineffective by a Pflüger's apparatus. Generally after every contraction the direction of the current was inverted. After every contraction the electro-magnet, indirectly dependent on the metronome, allowed the regulator of the clock-work of the myograph to make a half-revolution, and therewith the cylinder rotated a short distance. The heights of the contractions were drawn about 1 m.m. from each other. The course of the work of the muscles, which often wrote many hundred contractions before complete exhaustion, could thus be easily seen.

Having arranged this apparatus, the muscle, in unchanged intervals of time (2—12 secs.) lifts a weight (which must not exceed that of an entire frog, 50 grms.) from a support ("Ueberlastung"), then the difference between the heights of two contractions following each other—the difference of fatigue ("Ermüdungsdifferenz") —is constant, *the line connecting all the highest points of the equidistant series of contractions is straight* ("Ermüdungscurve"). *The line of fatigue falls more rapidly towards the abscissa the smaller the interval between the irritations* ("Ruhezeiten"), i.e. *the difference of fatigue diminishes when the intervals between the irritations increases.* A striking peculiarity of muscle is this, that the height of the contraction, observed at a given moment during the experiment, is quite independent of the quantity of work which it has

previously accomplished, only the *number* of irritations (maximal) which the muscle has previously received regulates the height of its present work. When the interval between the irritations is changed, the fatigue progresses during this tempo of the contractions, in the same manner as if all the contractions from the beginning onwards had been made with the same interval.

Let the interval be constant, but change the weight ("Ueberlastung"), the smaller weight is of course lifted higher than the larger one, but the terminal points of all the heights to which the muscle has raised the equal weights lie in a straight line, or, in other words, the difference of fatigue during unchanged intervals of irritation remains constant even when the overweightings ("Ueberlastungen") of the working muscle are changed. *The lines of fatigue of different overweights run parallel.*

If the weight is attached to the muscle so that it can stretch the muscle, even when the latter is at rest ("Belastung"), the line of fatigue remains, with equal intervals and weight, a straight one only to the point where it cuts the abscissa drawn by the inactive non-weighted muscle. From this point onwards—where the shortening of the active muscle is equal to the elongation of the passive one carrying the same weight ("Dehnungslänge")—the line of fatigue is a hyperbole in an algebraical formula: thus from where the shortening is equal to the above elongation (δ) the difference of fatigue (D) becomes always smaller with the number (n) of contractions, and is represented thus: $\frac{\delta^2}{n^2D}$, or, in other words, the line of fatigue straight to this point becomes thence nearly a hyperbola whose asymptote is the abscissa of the passive weighted muscle. This relation is easily explained when one assumes that the elasticity of an active muscle is quite as great as that of a muscle at rest. For the confirmation of this we must refer to p. 239—245. It is obvious that the line of fatigue of a stretched muscle ("belastet") cannot remain straight to the end of the work, because of the weight which is held in equilibrium by the elastic force of the passive stretched muscle, a smaller fraction requires to be lifted by the contractions, the smaller the shortenings are, so that the muscle in a manner contracts always with a smaller weight. There is thus explained the at first sight somewhat paradoxical phenomenon that a fatigued muscle (which can contract very little) can raise great weights the same relative height as light ones (the latter of course from an absolute higher position), the muscle being less stretched by a light weight.

We have not space to mention many other remarkable facts that are contained in the original, to which we must refer for further information. We can only remark that the author has found both in the muscles of dogs, and in those of living intact frogs, that the fundamental law of the constant difference of fatigue is valid, and by means of transfusion of different liquids through the vessels of the muscles has shown, that not only arterial blood of the same or different classes (rabbits' blood in frogs) can partly restore the fatigue of muscles without their requiring to rest, but that also a 0·5 per cent. solution

of NaCl, which contains a very small quantity (0·01 per cent.) of permanganate of potash can produce the same effect. The author cites an experiment in which he weighted a very exhausted gastrocnemius of a frog with 40 grms., and found after injection of this solution that 190 contractions were produced, which at the beginning were four times as high as those made by the muscle before the injection. There was at most in this case 0·018 milligr. of ozonized oxygen which the muscle could have extracted from the reduced outflowing fluid, an increase of work of 0·01176 kilogrammeter.

Pure solution of common salt (0·5 per cent.) had little or no effect.

Muscles.

Post-Mortem Rigidity of Muscles.—Michelsohn (*Inaug. Dissert.*, Dorpat, 1872) has arrived at the conclusion from experiments conducted under Schmidt's direction, that the coagulation of myosin post-mortem is brought about by a ferment.

Chemical Reaction of Active and Inactive Muscle.—Grützner (*Pflüg. Archiv*, 1873, vii. 254) tetanized the gastrocnemius muscle of a frog till it was exhausted, while the opposite muscle remained at rest under similar conditions with the active one. Each of the muscles was now triturated with 5 cm. of a half per cent solution of pyrogallic acid, and the product filtered. The active muscle yielded a clear or yellowish filtrate, the inactive one a brown one. In the one purpurogallin was formed, in the other none, or only a trace. The latter had been able to oxydise the pyrogallic acid, the former not. The browning in the second case is not to be ascribed to the strong alkaline reaction of the inactive muscle. When a mixture of pyrogallic acid, and chloride of iron is used, which is a reducing as well as an oxydising mixture, itself of a brownish red colour—with the inactive muscle a red-brown colour is obtained, and with the active a bright violet. This change the author ascribes to the large quantity of lactate of an alkali present in the active muscle, which the author found yielded a violet colour when treated with the above mixture.

"Electrotonus," Hermann, in *Pflüg. Arch.* vii. 301, 323 and 497.——"The Conditions of Equilibrium for Stimulated and Non-stimulated Muscles," Fuchs in *ibid.* 421.——"Contraction of Muscular Fibre," Krause in *Pflüg. Arch.* vii. 508.——"Action of Veratria on Muscular Fibre," Fick and Böhm in *Arb. aus d. Phys. Lab. d. Würz. Hochschule*, ii. 142.——"Electrotonus," Bernstein in *Pflüg. Arch.* viii. 40.——"Action of Electricity on Muscle and Nerve," Bernheim, in *ibid.* 60.

Muscular Irritability after Systemic Death.—Croonian Lecture. B. W. Richardson. (*Proc. Roy. Soc. Lond.* 339.)——The principal results obtained by the author are: 1. There are three degrees of muscular irritability—the active efficient, passive efficient, negative inefficient. The muscle after death may be suspended in

any of these conditions for action; but, as a rule, it is the last condition only that is maintained long after death. 2. Muscular irritability may be suspended or stopped altogether under three different conditions, having reference to its connection with nervous activity:—(*a*) the nervous muscular activities may be suspended equally, on which there may follow spontaneous return of motion. (*b*) The muscular irritability may outlive the nervous function, on which the phenomenon of irritability may be induced by the application of the motor forces, but there is no return of spontaneous irritability, i.e. of irritability belonging to the animal as a distinct agent. (*c*) The nervous function may outlive the muscular irritability, under which circumstances irritability is invariably stopped by the production of persistent contractility of the muscular fibres. 3. Nervous activity exciting muscular action is identical with all the motor forces, and particular to none. It is equivalent to mechanical, calorific or electrical force, and equally susceptible of manifestation through either. 4. Muscular irritability is possible after death. Cold, in certain defined degrees, suspends without destroying it. The motor forces strike it into rest. Blood sustains it or stops it according to the balance of powers existing between the muscular and nervous systems. Some chemical agents suspend it independently, others suspend it together and equally, with suspension of the nervous function. When suspension is equal there may be spontaneous return; when it is unequal there is no return.

On the so-called Shortening of Tendons.—Hermann (*Pflüg. Arch.* vii. 417) cites an experiment to show the connection between this condition and the coagulation of the albuminous substances contained in the tendon when a piece of tendon of a known length is placed in water; when the water is at 65° the length of the tendon is completely unchanged, but just at this point there begins a marked shortening, which is complete at 75°. This condition of the tendon takes place at exactly the same temperature at which ordinary albumen coagulates. (See reply to the above, Englemann in *Pflüg. Arch.* viii. 95).

Miscellaneous.

Reflex Movements of the Uterus.—Schlesinger (*Wien Med. Jahrb.*, 1873, i. Hft. 4). Electrical irritation of the central end of a spinal nerve produces after 5—15 seconds powerful general movements of the uterus. In a curarised rabbit, upon which tracheotomy has been performed and artificial respiration kept up, if the artificial respiration is suspended, the uterus exhibits energetic contractions. The same results when the central end of the median or crural nerve is electrically stimulated. The path of the reflex action is not through the spinal cord, for after section of the medulla, between the occipital bone and atlas, a continued irritation of the stump of a nerve for 40 seconds was without effect. So that the author believes that the path by which irritations going from the

brain reach the uterus, is at least partly through the nervous plexus of the aorta, but still this is not the only route by which the motor influence travels.

Influence of Changes in the Barometric Pressure.

ON THE PHENOMENA OF LIFE.—Bert. (*Comptes Rendus*, No. 8, Aug. 25, 1873, p. 531.) Twelfth note on this subject:—1st. When oxygen reaches the proportion of 28 to 30 vols. for 100 vols. of arterial blood in a dog, the animal is seized with convulsions, while a proportion of about 35 vols. proves fatal. 2nd. All these convulsions, however varied their type, are due to direct irritation of the spinal cord. From the outset of the convulsive attack, the temperature falls several degrees. There is therefore a profound alteration in nutrition, which does not take place in poisoning by convulsive substances, e.g. strychnine. These convulsions are to be regarded as epiphenomena manifestations of the spinal cord of general disturbance of the organism, such as happens in asphyxia or rapidly fatal hæmorrhages. To what alterations in the blood are we to attribute these strange results? Is it to be supposed that under a little more pressure oxygen forms with the blood-corpuscles a more stable combination than with ordinary hæmoglobin? a combination from which the tissues are not able to remove the oxygen of which they have need? This is not so, for scarcely had the animal been placed under normal pressure, when the excess of oxygen disappeared from the blood, whilst the convulsions often continued many hours and the temperature continued to fall. Nor is it likely that a substance so formed by the super-oxydation of the blood would persist after the return of air, the blood having thus become a toxic substance; for the author has injected with impunity considerable quantities of blood, charged with oxygen to the fatal extent, into dogs rendered nearly bloodless. All tends to show that the blood is an intermediary, carrying the blood to the tissues. The author believes that it is the excess of oxygen in the tissues themselves which alters the chemical phenomena of nutrition; the central nervous system is the first part affected by the sudden change in nutrition, hence the epiphenomenon of convulsions. Fishes die with convulsions when the water contains more than 10 vols. of oxygen. The toxic action appears also in the invertebrata. Insects die more rapidly in compressed oxygen than Arachnida and Myriapoda, and the latter more rapidly than Mollusca and Earthworms.

Regarding the general nature of the alteration of the nutritive phenomena, the most evident is a diminution in the intensity of the phenomena of oxydation. 1. If an animal is made to respire in a certain volume of air, in the normal state, and afterwards in air poisoned with oxygen, it is found to absorb much less oxygen in a given time during the second period than during the first. 2. On analysis of the arterial blood of a dog which had convulsions due to oxygen, and which had respired some time in open air, the

quantity of CO_2 is very small (25, 20, 15 vols. for 100 vols. blood.). 3. The proportion of urea falls considerably under the influence of compressed air. In one case it fell 12 grms. to 4 after seven hours, at 8 atmospheres. Similar results were obtained *in Vitro*. A fragment of muscle or other tissue separated from the body, absorbs in a given time less oxygen and forms less CO_2 in compressed air (the same is true of seeds). This diminution in oxydation is the cause and consequence of a retardation, and even remarkable arrest of the numerous chemical actions which are closely allied to those which take place in living beings. Fragments of muscle had not commenced to putrefy after eight days in oxygen, compressed to twenty-four atmospheres, while putrefaction was complete in four days under identical conditions. Glucose added to blood is destroyed much more slowly in compressed oxygen than at normal pressure. The same, though less marked, is the case in the transformation of starch into glucose by the saliva. Milk undergoes the lactic fermentation more slowly. It is therefore not astonishing that the nutritive actions in animals and vegetables are similarly arrested and death ensues.

But diminution in the intensity of the nutritive acts does not explain all. Slow asphyxia as well as low barometic pressure also diminish it, and yet do not give convulsions lasting for several hours, or disturbances that persist even when the quantity of oxygen absorbed in a given time is reduced to the normal. Seeds of barley arrested in their evolutions by a vacuum do not die, but they die in compressed air. There is then, in the physico-chemical acts of nutrition, not only a diminution in quantity, but also a modification in quality.

REPORT ON PHARMACOLOGY. By Dr Thomas R. Fraser.

Phosphorus.—In a research on the action of phosphorus (*Virchow's Archiv*, LV., June, 1872; and abstract, *Glasgow Medical Journal*, November, 1872), Wegner examined its effects on the stomach and liver, and on the osseous system. Moderate doses were found to produce in rabbits a catarrhal condition of the stomach, followed by great thickening and pigmentation of the mucous coat, and the formation of characteristic flat ulcers. By similar doses, the connective tissue of the liver was increased, and by its subsequent contraction the organ became cirrhotic. The observations on the effects of this substance on the osseous system are of great interest. It does not appear to have any selective influence on the bones of the jaws, for when the tibia was exposed to the action of phosphorus vapour, changes soon occurred in it. These changes appear to be the result of irritation of the periosteum, leading to increased formation of bone and to suppuration; and they are induced by administering phosphorus by the stomach as well as by bringing its vapour into direct contact with the periosteum. In growing animals to whom small doses of phosphorus were given by the stomach, its peculiar action on the osseous system was manifested by an unusual density and compactness of the bones. The cartilage of the young bones was quickly changed into true osseous tissue, and the periosteum formed new dense bone with unwonted rapidity. This action on the osseous system is not produced by phosphoric acid nor by the phosphates.

Silver-salts.—From several experiments on animals of many different classes, Rouget (*Archives de Physiologie*, No. 4, 1873, p. 333) finds that, without exception, the first symptoms produced by the absorption of silver are due to disturbances in the functions of the nervous and muscular systems, ranging from feebleness of the extremities, torpor, and somnolence, to complete loss of voluntary movement, convulsions, persistent spasm, and paralysis. The respiration is afterwards affected, its rate being gradually diminished until, in fatal cases, it ceases; and, at the same time, there occurs in many animals hyper-secretion of bronchial mucus, with an appearance after death of retraction, or diminution in the volume, of the lungs. The voluntary muscles are also directly affected, and in frogs and other animals their contractility may be destroyed before complete death. The circulatory system resists the action for the longest time, as the heart continues to contract after the function of the cerebro-spinal nervous system has been abolished, and even after the voluntary muscles are no longer contractile. In opposition to the opinion of Krahmer, Rabuteau and Mourier (and also to that of Bogolowsky, with whose elaborate research the author seems

unacquainted), Rouget maintains that silver does not alter either the composition or the properties of the blood. The greater number of the experiments were made with nitrate or chloride of silver dissolved in hyposulphite of soda.

ARSENIC.—F. Schäfer and Rudolf Boehm (*Verhandlungen der Physikal.-Med. Gesellschaft in Würzburg*, III. 3) believe that previous experiments have established (1) that pure metallic arsenic is not poisonous; (2) that no compound is formed between arsenious acid and albumen; (3) that arsenious acid exists as such in the urine and coagulum, not serum, of the blood of poisoned animals, in the latter case perhaps in combination with potash; and (4) that arsenic is able to prevent putrefaction. In order to determine if arsenic interferes with digestion, they compared the action upon white of egg of natural and artificial gastric juice to which no arsenious acid had been added, with portions of the same specimens of gastric juice to which small quantities of this poison had been added. They arrive at the important conclusion that arsenic is not able to prevent the digestion of albumen. From similar experiments, they arrive at the conclusion that the pancreatic digestion of albumen is also unchecked by this substance.—From an experiment on a dog to which arsenic was administered for several days, and chemical analysis made of the urine and fæces previously to, coincidently with, and subsequently to the administration, H. Von Boeck (see *New York Medical Journal*, XVI. 1872, p. 301) concludes that the administration of arsenic exerts no essential influence upon the amount of nitrogen excreted, nor upon the decomposition of albuminoid substances.

BISMUTH.—On undertaking a series of researches that have yielded many important results, on the presence of various metals in the tissues, Mayençon and Bergeret have found it necessary to plan a new method of procedure, those already existing being too tedious and difficult of application (Robin's *Journal de l'Anatomie et de la Physiologie*, No. 3, 1873). They simply treat the substance or tissue with a strong acid, and insert into the product a voltaic pair, with platinum as one of its constituents. In this way, the metal is deposited on the platinum, from which it may be removed and tested by converting it into a chloride. Applying this electrolytic method to determine if bismuth is absorbed into the tissues of man and the lower animals to whom subnitrate was administered by the stomach, Mayençon and Bergeret conclude (1) that subnitrate of bismuth is promptly absorbed, and deeply impregnates the tissues; (2) that it is slowly eliminated; and also (3) that it seems to increase the plasticity of the blood.

CALCIUM-SALTS.—On numerous occasions, Rabutreau has enunciated the law, based on his own experiments and those of others, that the activity of metallic compounds increases according as the atomic weight of the metal is more elevated. He has recently made some experiments to determine if the calcium-salts adhere to this

law (*Comptes Rendus*, 10 Février, 1873). When the chloride in solution was injected into the circulation of a dog, it caused death if the dose was such that the calcium present in the salt was about the same in weight as the potassium in a dose of chloride of potassium previously found sufficient to cause the same effect. Calcium, therefore, forms no exception to the above law, its atomic weight being 40, while that of potassium is 39. When administered in this way, calcium causes death by arresting the heart's contractions in diastole, just as potassium does. Further, it resembles all other metallic substances, excepting sodium and lithium, in acting as a muscular poison.

MERCURY.—Adopting with mercury the method of research described in the abstract of their paper on bismuth (see *ante*), Mayençon and Bergeret (Robin's *Journal de l'Anatomie et de la Physiologie*, No. 1, 1873, p. 81, and No. 3, p. 233) have determined: —(1) That mercury and its salts are absorbed by the skin as well as by the stomach. (2) That of the absorbed mercury, the greater part is immediately eliminated, while only a small portion is deposited in the tissues, from which it is very slowly eliminated. (3) That elimination seems to occur through all the excretory fluids (urine, fæces, saliva, milk, spermatic fluid), but chiefly through the urine and intestinal juices. (4) That iodide of potassium has a marked action in freeing the system of deposited mercury. In the course of their research, they found that perchloride of mercury excites an abundant secretion of bile.——In a case of acute poisoning by perchloride of mercury, A. Olivier (*Archives de Physiologie*, No. 5, 1873, p. 547) observed a remarkable lowering of temperature at the commencement of the poisoning, and the occurrence of albuminous nephritis indicated by the presence in the urine of albumen, tumefied renal cells, and granular, fatty, and hyaline cylinders. Contrary to the reported observations of Salkowsky and Bouchard, the urine was altogether free from sugar.

LEAD, GOLD.—Mayençon and Bergeret have applied with some modifications their method of analysis (see Bismuth) to a research on the absorption and elimination of lead, with the following results: —(1) Salts of lead are not absorbed by the skin. (2) They are absorbed slowly and in small quantity by the intestine. (3) Absorbed lead seems to be deposited principally in the liver and spleen. (4) Sometimes, after long-continued administration, the kidneys and urine contain traces of this metal. (5) The elimination is prompt and complete, and is chiefly effected through the liver.—— A similar research on gold has yielded the same observers only doubtful results (*Op. cit.*, p. 226).

CHLORIDE OF SODIUM.—Falck (*Virchow's Archiv*, Vol. LVI. p. 315) finds that chloride of sodium is a more active poison for mammals than is generally supposed, from three to five parts to every thousand of the animal's weight producing death in dogs, when injected into the veins. It is more rapidly eliminated when injected into a vein than

when introduced into the stomach, and during its elimination it greatly increases the quantity of urine and so probably causes thirst. In fatal doses, frothy liquid escapes from the mouth and nose, the heart's contractions are weakened, and tremors and convulsions precede death. The most conspicuous post-mortem appearances are pallor of the muscles, dilatation of the heart, dark loosely coagulated blood, frothy liquid in the bronchi, œdema of the lungs, congestion of the liver and spleen, and mucous fluid in the stomach. Sodium-phosphate was found to be less poisonous than chloride; it acts less powerfully on the heart, and does not produce exudation into the bronchi, nor œdema of the lungs.

Alcohol and Absinth.—The comparative effects of alcohol and absinth have for some time been carefully studied in France. In a recent contribution, Magnan (*Archives de Physiologie*, No. 2, 1873, p. 145, and No. 3, p. 281) shows that the prolonged administration to dogs of somewhat large doses of alcohol produces symptoms closely resembling those of chronic alcoholism in man. On about the fifteenth day of intoxication, irritability and excitability occur; two days afterwards, there are illusions and hallucinations at night; and at the end of the month, delirium both day and night. Should the administration be prolonged into the second month, tremors make their appearance in the posterior extremities, and extend from them to the anterior extremities and then to all parts of the body. The animal finally succumbs to disorders of digestion and other complications, which recall the accidents that terminate alcoholic poisoning in man. After death, dogs present, in various degrees, steatosis of the liver, kidneys, and heart, and results of chronic irritation of the meninges, cord, and pericardium. Magnan finds that the symptoms of poisoning by absinth are quite distinguishable from those of poisoning by alcohol. Even in small single doses, the former substance causes, in dogs, vertigo and muscular twitchings in the anterior parts of the body; while in large doses, it causes epilepsy and delirium. By a number of careful experiments, he has determined that during the stage of tonic spasm in absinth-epilepsy, the pupils dilate, the retina becomes injected, and congestion occurs in the brain—phenomena that do not accord with the generally received theories of the mechanism of epilepsy.—Accepting the opinion of Beale, Binz, and others that alcohol checks the movements of white blood-corpuscles and other masses of protoplasm, Ross endeavours to show how on this opinion a theory of the action of alcohol may be founded, sufficient to explain its effects in health and disease (*British Medical Journal*, October 4 and 25, 1873).

Chloral.—Arndt (*Archiv für Psych. und Nervenkrank*, Bd. iii., Hft. 3, 1872) and Gubler (*Journal de Pharmacie et de Chimie*, July and August, 1873) refer the poisonous symptoms that have been observed in many cases of chloral-administration, to an injurious action on the vaso-motor nerves and the heart. Gubler also advances many arguments against Liebreich's theory of the action of chloral being due to the production from it of chloroform in the blood.

Nitrous Oxide.—The effects of nitrous oxide on plants and animals have been carefully studied by Jolyet and Blanche (*Archives de Physiologie*, No. 4, 1873, p. 364). They find that when chemically pure it prevents the germination of seeds, as those of barley and cress; and acts as an asphyxiating gas on animals, death being produced with all the signs of asphyxia by strangulation or by the respiration of inert gases such as hydrogen or nitrogen, and in about the same time. If it produce anæsthesia when respired pure, it is by bringing about an insufficiency of oxygen in the blood; and insensibility commences to show itself only when the arterial blood contains no more than 2 or 3 per cent. of oxygen. When this occurs, the arterial blood is very dark in colour, and it contains from 30 to 40 per cent. of nitrous oxide. Animals may, however, live if they are made to respire a mixture of nitrous oxide and oxygen in the proportion of atmospheric air, the nitrogen being replaced by nitrous oxide: but they do not then exhibit any symptoms of disturbed sensibility, although the arterial blood may contain from 30 to 35 per cent. of nitrous oxide. The absence of oxygen seems to be a necessary condition before anæsthesia results, nitrous oxide being itself quite irrespirable.

Nitrite of Amyl.—In a long paper on the action of nitrite of amyl (*Archives de Physiologie*, No. 5, 1873, p. 467), which contains an account of numerous experiments in which this substance was administered to various animals by inhalation, subcutaneous injection, and injection into veins, Amez-Droz arrives at conclusions that are generally in accordance with the opinions usually held. General uneasiness, great increase in the rate of the heart's contractions, dilatation of blood-vessels with reduced blood-tension, irregularity of respiration, lowering of temperature, and paralysis, when the doses are comparatively small; with the addition of convulsive movements, passage of fæces and urine, reduction following increase in the rate of the heart's contractions, and finally, coma and death, when the doses are large—are the chief symptoms to which he draws attention. His experiments do not support Richardson's statement that the paralysis results from an action on motor-nerves, but they lead him to refer this effect to a stupefying action on the nerve-centres. Amez-Droz also differs from Brunton in his explanation of the remarkable dilatation of blood-vessels produced by nitrate of amyl. He argues that it cannot be a result of paralysis of the muscles of the minute vessels, because after dilatation has been produced he has frequently observed that the vessels contract momentarily during movements of the animal, and again dilate when these movements have ceased—changes of calibre that could not occur were the muscular sheath of the blood-vessels paralysed. He believes it to be more in accordance with observation to suppose that by increasing the quantity of carbonic acid in the blood, nitrite of amyl renders this fluid sufficiently irritating to enable it to stimulate the peripheral ramifications of the vaso-motor nerves, and thus to produce a reflex dilatation of the vessels. (No sufficient

experimental proof of this theory, however, is advanced, and, besides, it is well known that the changes in the minute blood-vessels during asphyxia are not similar to those produced by nitrite of amyl. Rep).——F. A. Hoffmann (*Reichert und Du Bois Reymond's Archiv*, 1872, Heft 6, p. 746; and *Centralblatt*, 1873, No. 36, p. 374) has made an important addition to our knowledge of the effects of nitrite of amyl. He injected under the skin of rabbits doses varying from 0·45 to 0·66 gramme (7 to 10 grains). The quantity of urine voided was increased, and in 24 hours it was double the normal amount, and contained from 1·0 to 2·5 per cent. of sugar, which entirely disappeared in from 12 to 30 hours. Diabetes could be produced several times in the same animal, but if the injections followed each other too quickly, the animal died.

UREA.—From experiments made on himself, Rabuteau (*L'Union Médicale*, Tom. XIV., 1872, p. 841) has found that when urea is introduced into the organism it is entirely eliminated by the urine, without undergoing any change, within twenty-four hours. It may also be detected in the saliva, which, however, normally contains a small quantity. Contrary to the usually held opinion, Rabuteau maintains that urea has only a very feeble diuretic action.

BENZINE, NITRO-GLYCERINE, NITRIC AND SULPHURIC ACIDS.—In an interesting contribution to the toxicology of these substances, Starkow (*Virchow's Archiv*, LII. No. 4) remarks that by replacing the hydrogen in carbo-hydrogens by the radicle NO_2, their action is modified and their toxicity increased. At the same time, the nitrous products confer upon the blood a new modification which is not produced by the carbo-hydrogens. Thus, in all animals poisoned by binitro-benzine, the blood invariably exhibits, besides the two oxyhæmoglobine lines, an absorption line at the boundary of the orange and red of the spectrum, and corresponding to Frauenhofer's line C. The same line is produced in acid hæmatine by nitro-benzine, nitro-aniline, and nitro-naphthaline, in all of which a single H is replaced by NO_2, but not markedly in the blood of animals poisoned by these substances. Corresponding to these characters, the toxic action of the latter substances is much less powerful than that of binitro-benzine. Chloro-benzine and benzine are greatly exceeded in the intensity of their action by nitro-benzine, and they have not the above action on the blood. They, however, are able to separate crystals of hæmoglobine from the blood to which they are added, and as this property is especially possessed by chloro-benzine, it may be employed in place of ether to separate blood-crystals. Binitro-benzine presents the peculiarity of having a toxic power which is not related to its solubility. The action of nitro-glycerine on the blood-pigment is analogous to that of nitro-benzine, and the two substances are nearly equal in toxicity. Nitric and sulphuric acids have only partly this action on the blood. Spectrum-analysis of the blood of animals poisoned by them shows the line of acid hæmatine, which is not produced by hydro-chloric, phosphoric, and other acids. Absorbed by the stomach, they seriously modify the blood, and act as poisons

as much by this property as by their local caustic action. Starkow is inclined to disagree with Letheby's opinion of the possibility of nitro-benzine becoming transformed in the organism into aniline. —The occurrence of this transformation is also denied by Lehmann (*Journal de Pharmacie et de Chemie*, Octobre, 1873, p. 335) in some observations appended to an account of a case of poisoning by nitro-benzine. The chief symptoms observed were cephalgia, vertigo, unsteady movements, cyanosis, and a strong odour of bitter almonds.

ACONITIA.—Rudolf Boehm and L. Wartmann have published a valuable research on the physiological action of Merck's amorphous aconitia (*Verhandl. der Physikal-Medic. Gesellschaft in Würzburg*, III., Hft. 1, 1872). They did nót succeed in confirming Achscharumow's statement, that aconitia paralyses the terminations of the motor nerves (see *Journal of Anatomy and Physiology*, Vol. I., 1867, p. 154), nor the same statement in reference to crystalline aconitia more recently made by Gréhant and Duquesnel (see *Journal of Anatomy and Physiology*, Vol. VI., 1872, p. 496). On the contrary, their experiments have led them to conclude that paralysis is produced by this substance, by an action on the cord, whereby diminution in the activity of the sensory ganglia, with resulting impairment of reflex sensibility, is in the first place induced, and is succeeded by diminished excitability of the motor ganglia with total paralysis of voluntary movement. The only effect observed by them on the peripheral terminations of the motor nerves, was an irritative one, resulting in fibrillary muscular contractions. The muscles were otherwise unaffected. A large portion of their research is devoted to the action of aconitia on the circulation. They found that in large doses it diminished the frequence of the heart's contractions, and finally caused complete stoppage of the heart's action in diastole, before which, however, a temporary acceleration occurred in the rate of contraction. The mean pressure of the blood was a little increased at the commencement of the poisoning in rabbits, but it was always lessened in dogs and cats; and at a later stage, in all animals, it was progressively lessened. Their experiments lead them to conclude that the effects on the heart are due to the excitation of the inhibitory apparatus situated in the heart, and not of the central origin of the vagi as Achscharumow holds. The lowered blood-tension is due in the first instance to paralysis of the afferent vascular nerves, and afterwards to a further paralysis of the vaso-motor centre.

APOMORPHIA.—Quehl (*Ueber die Physiologischen Wirkungen des Apomorphins*. Inaug. Dissert., Halle, 1872; Abstract in *Lond. Med. Rec.*, January 22, 1873, p. 44), under the direction of Köhler, confirms many of Siebert's previous observations regarding the emetic action of apomorphia (see *Journal of Anatomy and Physiology*, November, 1872, p. 194). He differs from Siebert in stating that division of the vagi prevents this action. During five weeks, he produced vomiting at least once a day in a dog, by the administration of apomorphia, and found that no tolerance occurred, and that the health was not injured, the dog having actually gained 2½lbs. at the end

of that time. It would appear that only small doses produce vomiting in dogs, when it is administered subcutaneously. Somewhat large doses (3 grs. or more) do not produce vomiting, but symptoms of general poisoning such as staggering, weakness of the posterior extremities, great salivation, and partial paralysis, during which the animal lies extended and executes natatory movements. After death from a fatal dose, no special appearances are seen. Quehl further states that apomorphia has no action on sensory or motor nerves, voluntary or involuntary muscles, nor on the blood-pressure. Chloroform-narcosis prevents its emetic action.

HYDROCOTARNIN.—Hydrocotarnin is one of the opium alkaloids discovered by O. Hesse (*Ann. der Chem. u. Pharm.*, Supplbd. VIII.), who assigned to it the formula $C_{12} H_{15} NO_3$. A somewhat fragmentary investigation on its action has been published by F. A. Falck (*Viertel-Jahrschrift f. Gericht. Med.*, Januar., 1873, 49, and *Inaugural Dissertation*, Marburg, 1872). With the chlorhydrate, this observer has made two series of experiments; one on frogs, and the other on rabbits. He defines the fatal dose for the latter as three grains per kilogramme of animal, and for the former, about ten times this dose per kilogramme. This alkaloid has therefore a toxic activity less than that of thebaia and codeia, and greater than that of morphia. It produces, like codeia and other opium alkaloids, two classes of symptoms in rabbits; a tetanic and a narcotic. In frogs, the symptoms belonging to the tetanic variety predominate, but before death a stage occurs, in which the motor nerves are paralysed, and the heart alone remains active. Falck has made the interesting observation, that hydrocotarnin possesses, in common with atropia and codeia, the power of causing the heart in frogs to resume its contractions after it has been brought to rest in diastole by muscaria.

RHUS VENENATA AND R. TOXICODENDRON.—J. C. White publishes a valuable paper on the action of *Rhus Venenata* and *R. Toxicodendron* upon the human skin (*New York Medical Journal*, March 1873, 225). He describes minutely a number of cases, including his own, in which he observed the effects on the skin produced by contact with these plants. His opinion is, that the eruption is of an eczematous, and not of an erysipelatous nature, that it is not capable of being transferred by contact or otherwise from one person to another, and that it is produced by a volatile poison (probably the toxicodendric acid of Maiseh), which may be conveyed to the skin by exhalations from the plant as well as by direct contact with it.

QUINIA.—The numerous and valuable papers of Binz on the action of quinia have been increased by a recent important addition (Ueber Chinin und Blut: *Archiv für experimentale Pathologie und Pharmakologie*, Bd. I., Heft I., 1873, p. 18). This paper treats chiefly of the effects of the alkaloid upon the oxidation changes of the blood. It is shown to arrest the formation of acid in the blood-clot, to diminish the power of blood to carry ozone, and to lessen oxidation. The previous experiments of this observer had led

him to conclude that quinia retards, and even prevents, the movements of white blood-corpuscles. In the present paper this is attributed to the diminished power of oxidation by the red corpuscles produced by the action of quinia on hæmoglobin, a liberal supply of oxygen being necessary for the migratory movements of the white cells. The principal action of quinia would therefore appear to be a lessening of oxidation; in virtue of which the temperature is lowered, the quantity of nitrogen excreted diminished, and the movements of the colourless blood-corpuscles retarded or paralysed. It has besides a remarkable power in preventing putrefaction and destroying low forms of organism, regarding which Binz has made many valuable observations.—This parasiticide action of quinia is treated of in the first part of a paper by Rochefontaine (*Archives de Physiologie*, No. 4. 1873, p. 389); but the report of this paper will be deferred until its publication is completed.—The absorption of quinia, and the transformation undergone by it in the system, are discussed by Guyochin (*Thèse de Paris*, 1872; and *Revue de Sciences Médicales*, Tome I., No. 1, 1873, p. 283). By means of the double iodide of potassium and mercury, he has discovered the presence of quinia not only in the urine, but also in the saliva, blood, and intestinal contents. He differs from Kerner's opinion that this alkaloid is transformed in the blood into a new base, and maintains that it is easy to establish, by applying suitable reactions, that the supposed new base is quinidine.

THEIN, CAFFEIN, GUARANIN, COCAÏN, THEOBROMIN.—The action of these substances has been carefully investigated by numerous experiments on warm- and cold-blooded animals, by A. Bennett (*Edinburgh Medical Journal*, October, 1873, p. 323). His investigation was mainly directed to determine the symptoms produced by their administration, and the causation of their effects on the muscular and nervous systems. He finds that these five principles are to all appearance identical in their physiological action. They cause, in small doses, cerebral excitement, not succeeded by coma, with partial loss of sensibility; and in large doses, cerebral excitement, complete paralysis of sensibility, and tetanic spasms and convulsions. The influence on sensibility is explained by an action on the posterior columns of the cord, and on the entire system of peripheral sensory nerves, both of which are paralysed by large doses. The anterior columns of the cord, the peripheral motor nerves, and the striped muscles do not seem to have their functional activity diminished; but an irritant action is produced on the cord, which shows itself by the occurrence of spontaneous spasms. The vaso-motor system is decidedly affected, contraction of the vessels being first produced, and afterwards dilatation with stasis of the blood. Further, these substances contract the pupils, lower and then elevate the temperature, increase salivary secretion, and induce a peculiar form of tenesmus accompanied with a copious discharge of clear mucus from the bowels.

ATROPIA.—From the results of several experiments in which atropia caused great increase of the pulse-rate, after the vagi had

been divided, H. C. Wood (*American Journal of the Medical Sciences*, April, 1873, p. 332) is inclined to think that it possesses a stimulant power upon either the accelerator nerves of the heart or their centres, and that the increase of the pulse-rate is not therefore entirely due to paralysis of the peripheral vagi. In a subsequent part of the same communication, Wood states his decided conviction of the *medical value* of atropia in opium poisoning. Although its action is not completely antagonistic to that of morphia, still it exerts valuable counter-effects, and notably upon the respiration, which it directly and powerfully stimulates.—Schiff's experiments (*La Nazione*, No. 235, Augosto, 1872) have established that the sensibility of the heart is lessened by doses of atropia, rather greater than are necessary to dilate the pupils, to such an extent, that the pulse-rate is neither accelerated nor diminished by increasing the blood-pressure to three or four times its normal amount, or by lowering it greatly.

NICOTIA, TOBACCO-SMOKE. — Basch and Oser (*Wiener Med. Jahrbuch*, 1872, p. 367) have investigated the effects of nicotia upon the intestinal movements, and the relations of these effects to the changes produced in the circulation. They find that nicotia produces movements of three kinds. The first two, already made known by Nasse, occur soon after the administration, consist at the commencement of only slight movements lasting a very short time, and then pass into tetanic contractions. Then occurs a period of repose extending over four or seven minutes, after which the intestinal peristalsis becomes more and more powerful, but retains its normal characters. Coincidently with the first and second kinds of movement, the pulse-rate is slowed by excitation of the vagi, and the blood-pressure is lowered, and the vessels dilated. During the third kind of movement the vagi continue to be excited, but the vaso-motor system is stimulated, and the vessels of the intestines contracted and pale. These intestinal movements are followed by a period of repose, during which the excitation of the vagi is diminished, the pulse-rate increased, and the intestinal vessels again dilated. These effects on the intestinal movements were found to be caused by an action of nicotia on the walls of the intestines aided by the changes produced upon the circulation. The latter are the combined result of its influence on the vaso-motor centre, and on peripheral vaso-motor nerves.——In a preliminary communication (*Centralblatt*, No. 41, 1872, p. 641), E. Heubel describes the method followed, and some of the results obtâined, in an investigation on the composition of tobacco-smoke, which he has carried on under the direction of Rosenthal. Since the researches of Vohl, Eulenburg and others, it has been generally supposed that tobacco-smoke is nearly or altogether devoid of nicotia, the temperature of the burning tobacco being sufficient to dissipate or decompose this alkaloid. Heubel imitated the process of smoking by drawing air through burning tobacco by means of an aspirator. Twenty-five cigars were employed in one experiment, and the smoke was passed through a Liebig's condensor, and then through water, and dilute solutions of sulphuric

acid and caustic potash. The products were submitted to physiological and chemical examination, with the result that a large quantity of nicotia was proved to be present in them. To explain the difference thus shown to occur between the results of submitting nicotia and tobacco to elevated temperatures, the author maintains that in the tobacco nicotia is present not in the free state but as a salt. This salt is able to be conveyed in tobacco-smoke without undergoing much change, although free nicotia is decomposed when subjected to an equally elevated temperature.

DIGITALIN.—The researches of numerous observers seem now to have established that digitalis and digitalin produce on the heart, first a slowing of its contractions, then an acceleration, and finally again a slowing terminating in paralysis of this organ if the dose be a sufficiently large one. Coincidently with these effects, there occurs an increase of blood-tension, which gives place to a diminution when, at the end of the last stage, the heart's contractions are slow and irregular. These phenomena have received various interpretations. Traube refers the primary slowing of the heart to an excitation of the vagi nerves, and this view is supported in an elaborate paper by Ackermann (*Deutsches Archiv für Klinische Medicin*, Vol. XI. December, 1872, p. 125), who shows that if the vagi be paralysed by atropia, digitalin no longer slows the pulse. This observer has also found that during the stage of cardiac acceleration following slowing under the action of digitalis, the vagi nerves have completely lost their inhibitory power over the heart, and in this respect also he supports Traube's view that the acceleration produced by digitalis is a result of paralysis of the vagi nerves. He maintains, however, that it is likewise in all probability a result of a stimulation of the accelerating cardiac nerves. The slowing and arrest of the heart, which subsequently occur, are brought about by a totally different mechanism, namely, a direct action of digitalin on the muscular substance of the heart. In investigating the cause of the increase of blood-tension, Ackermann divided the spinal cord in the neck before administering digitalin, and found that the blood-pressure nevertheless increased. This substance, therefore, seems to produce contraction of the arteries independently of any influence on the vaso-motor centre, and the probabilities are that its action on the blood-vessels is due to excitation of the peripheral terminations of the vaso-motor nerves. It cannot be caused by an action on the heart, because blood-tension remains increased alike during acceleration and slowing of the contractions of that organ. Besides these effects, the temperature of the interior of the body falls, while that of the exterior rises, for a short time, coinciding with the increase of blood-tension. Ackermann suggests that this is due to the accelerated circulation causing an increase of temperature at the surface, and a cooling of the interior of the body.——Ackermann's views are opposed in a very ingenious and valuable paper by Boehm (*Dorpater medicinische Zeitschrift*, IV. 1, 1873) who maintains that the effects of digitalin on the circulation are the result of a direct para-

lysing action upon the heart, and a stimulating action upon the vaso-motor centre in the medulla.——Mégevand (*Action de la Digitale*, Paris, 1872), among many other experiments, administered to a rabbit a small dose of Nativelle's crystalline digitalin, and then divided the vagi, and afterwards the right cervical sympathetic. Proof was thus obtained that after the administration of digitalin section of the vagi greatly increases the cardiac pulsations without materially affecting the blood-tension, and that division of one cervical sympathetic causes a considerable diminution of blood-pressure, without influencing the heart's contractions to any important extent. He, therefore, concludes that digitalin directly excites the central origin of the vaso-motor nerves, and affects the vagi nerve-centres. The acceleration of the heart's beats and lowering of blood-tension, which follow large doses, are referred to paralysis of the vagi and vaso-motor nerves. In a subsequent portion of his paper, Mégevand advances evidence to show that digitalin diminishes the excretion of urea by lessening denutrition and organic combustion, and lowers the respiratory activity; and he believes that the diminution of temperature results from these two actions.

VERATRIA. — Claus (*Experimentelle Studien ueber die Temperaturverhältnisse bei einigen Intoxicationen*, Marburg, 1872) finds that a toxic dose of veratria causes, first, a slight fall in the temperature, then a rise to about the normal point, and finally a fall immediately before death. Sabadillin, on the contrary, produced effects nearly opposite to these, and even at the moment of death the temperature was above the normal.——The report of a valuable paper by Fick and Boehm, on the action of veratria on muscle, must be postponed.

ERGOTIN.—The researches of Holmes (see *Journal of Anatomy and Physiology*, Vol. v. 1871, p. 206) and others have established that ergotin contracts the minute vessels by a direct action on their walls; and it has been supposed by many pharmacologists that uterine contractions are merely the secondary result of the diminished calibre of the vessels of the uterus. A recent investigation by A. Wernich (*Virchow's Archiv*, Vol. LVI. 1872, p. 505) throws considerable doubt on this supposition. Having injected ergotin into the veins of female cats and dogs, he found that the first effect was contraction of the arterial capillaries, accompanied with venous congestion of the skin, muscles, intestines, bladder, and vascular covering of the brain and cord. As this effect was not prevented by section of the sympathetics, Wernich's experiments confirm the opinion of Holmes that it is the result of a direct action of ergotin on the blood-vessels. The blood-vessels of the uterus, however, were not observed to become contracted at the time when the other blood-vessels became so. Indeed, no change occurred in the uterus until the blood-vessels of other parts of the body were already diminished in calibre; it then itself began to contract, and coincidently with its contractions, anæmia occurred in its substance, as a result of obliteration of the lumen of its vessels. Section of the spinal cord between the

third and fifth dorsal vertebræ completely prevented the action of ergotin on the uterus. It is, therefore, probably due to stimulation (anaemic ?) of the centre of uterine movement in the brain or upper part of the cord, and not to a direct action upon the nerves or vessels in the uterus, nor upon its muscular fibres.——In a later communication (*Centralblatt*, No. 15, 1873, p. 353), Wernich draws attention to a matter of considerable practical importance relative to the administration of ergot in labour. In the autopsies of individuals who have perished under its influence, the bladder has habitually been found distended. This has been regarded as the result of retention caused by spasm of the sphincter, and the supposition has supplied an indication for the use of ergot in incontinence due to paralysis of this muscle. Wernich, however, finds that in animals poisoned by ergot the bladder quickly becomes refilled after being emptied with the catheter. The secretion of urine is therefore augmented by this substance, doubtless on account of its action on blood-pressure. The distended bladder which is thereby caused has frequently been accountable for retarded parturition. Wernich advises repeated catheterization in all cases where ergot is used during labour.

INFLUENCE OF CERTAIN SUBSTANCES UPON REFLEX EXCITABILITY.—Meihwizeen (*Pflüger's Archiv*, Vol. VII. 1873, p. 201) has made some experiments on the above subject. The spinal cord was divided behind the ears in frogs, the animals were then suspended by the lower jaw, the reflex excitability was tested by measuring the time that elapsed between the dipping of one foot into 0.2 per cent. solution of sulphuric acid and the subsequent movement that occurred, the substance to be tested was injected under the skin of the back, and the reflex excitability again tested at intervals. The results were as follows:—*Bromide of potassium*, in doses of from 5 to 10 milligrammes, lessened the excitability, and doses greater than 20 mgm. killed the animal. *Bromide of sodium* seemed to have no effect on spinal reflex excitability, even when given in doses of 60 mgm. *Acetate of zinc*, in doses of from 10 to 20 mgm., almost always proved fatal, and its action was a central one. *Chloral*, in large doses, reduces reflex excitability, but in small doses, 0.0005 grm., it has no effect, or produced only a slight diminution, which was not seen to be preceded by any increase as Radziejewski affirms. *Strychnia* increases the reflex excitability for mechanical but not for chemical excitants. *Caffein*, in doses of from 5 to 10 mgm., reduces the excitability for chemical stimulation. Doses larger than the above produce death in a few hours. The reflex centres in the spinal cord are paralysed; but the author has not observed the tetanus of the extremities described by Loven. *Morphia*, in doses of from 5 to 8 mgm. at first diminishes reflex excitability, which, after about three hours, returns to its normal state; and afterwards it increases it so that it becomes markedly exaggerated during from 12 to 24 hours after the injection. This increase is rendered apparent by chemical but not by mechanical stimulation, and when it has become developed, spasms occur. On

the increase giving place to diminution, however, the spasms still continue, with unabated frequency and strength. *Quinia* seemed to affect reflex excitability through its influence on the circulation. Doses of from 4 to 6 mgm. slowed and weakened the heart in about ten minutes, and altogether stopped it a few minutes afterwards; but the reflex excitability was not modified until from fifteen to thirty minutes after the heart's contractions had become feeble. *Alcohol*, in a dose of 1 cc. of a 10 per cent. solution, for a long period produced great diminution of excitability, but afterwards great increase, by a central action. With *digitalin*, Meihwizeen observed the same phenomena as Weil (see *Journal of Anatomy and Physiology*, Vol. VI. 1872, p. 500). In frogs, which the day previously had had the cerebral hemispheres removed, 1 mgm. of digitalin produced a marked decrease of reflex excitability, and this before any obvious effect upon the circulation. After section of the cord behind the tympanum the excitability returns to its former state. In frogs, with both brain and medulla removed, no direct depression is produced, only an indirect one after the circulation is weakened. The author does not adhere to Weil's explanation of the depression being produced by excitation of an inhibitory centre for reflex activity. He thinks it may be satisfactorily explained as a result of the action of digitalin on the circulation.

ACTION OF CONVULSANTS.—Several substances appear to combine a paralysing with a convulsant action. It occurred to H. C. Wood (*An investigation into the Action of Convulsants*, pamphlet) that the latter action may have a cerebral origin. Selecting aconite, prussic acid, veratrum viride, and impure veratria, as examples of such substances, he administered them to frogs and rabbits in which the spinal cord had been divided; and found that, with the exception of the impure veratria, they all failed to produce convulsions in the parts connected with the lower portion of the divided cord. Their convulsant action, therefore, is not of spinal origin. These various substances further are powerful modifiers of the circulation, and they probably diminish the quantity of blood in the brain. Wood describes several ingenious experiments which lead him to believe that the convulsant action is of brain origin, occurring only after the circulation is profoundly affected, and probably resulting from deranged cerebral circulation.

ACTION OF CERTAIN EMETICS.—An interesting paper by A. E. D'Ornellas (*Bulletin Général de Thérapeutique*, 15 Mars 1873, p. 193) on the physiology of vomiting includes an examination of the emetic action of emetia and tartar emetic. The conclusion is arrived at that when these substances are injected under the skin, the vomiting they produce is consecutive to their elimination by the stomach. This is founded on the observations, that they require about three times longer time to produce vomiting when they are administered by subcutaneous injection than by the stomach; that the time of vomiting coincides with that of elimination by the stomach; that such doses as cause vomiting when subcutaneously injected also cause inflammatory

lesions of the stomach and intestines, while such as are too small to cause vomiting produce no gastric nor intestinal lesions; and that, as determined by Kleiman and Simonowitsch, when tartar emetic is injected into a vein, vomiting is produced in a longer time than when it is introduced into the stomach, while in the former case antimony may be detected in the matters first vomited. D'Ornellas further believes that, in general, the excitation to vomit caused by emetics is the result of a direct action upon the peripheral terminations of the vagi in the stomach. (However the last statement may accord with observations referring to emetia and tartar emetic, it cannot be extended to the whole class of emetics. Of the other members of this class of which we have sufficient knowledge, there are several, and notably apomorphia (see *ante*), regarding which statements cannot be advanced in harmony with those on which D'Ornellas' last general conclusion is founded—*Reporter.*)

ACTION OF PURGATIVES.—Moreau, in opposition to Thiry and Radziejewski, was led by experiment to maintain (see *Journal of Anatomy and Physiology*, Vol. v. 1871, p. 201) that saline purgatives do not increase intestinal peristalsis, but modify osmosis in such a manner as to cause a great increase in the fluid contents of the intestines. This opinion is supported by Vulpian, who has experimented on curarised and morphised dogs, by injecting solutions of several purgative substances into the small intestine, and noting the changes during life and after death (*Bulletin Général de Thérapeutique*, No. 11, Vol. LXXXIV. 1873, p. 522). Sulphate of magnesia was found to rapidly increase the intestinal secretion, producing distension and redness of the gut, but no increased peristalsis; and after death, the production of a true intesinal catarrh was substantiated. A small portion of the injected sulphate was absorbed, and its presence could be detected in the urine soon after purgation occurred. Jalap produced a more intense catarrhal condition, chiefly apparent, however, in the large intestine; but it also slightly increased peristalsis.

ANTAGONISM.—(*Between Saponin and Digitalin.*) From a previous elaborate and valuable research (*Die lokale Anaesthesirung durch Saponin,* 1873) H. Koehler had been led to conclude that saponin, (*a*) paralyses the respiratory centre in the medulla, (*b*) produces temporary stimulation followed by paralysis of the vaso-motor centre, and (*c*) paralyses completely the cardiac nerves and muscular substance. Before its muscular contractility is destroyed, the heart, under the influence of this substance, acts in the same way as after section of the cardiac branches of the vagi and the sympathetics, and cannot be excited except through its intracardiac nerves. In many respects, therefore, the action of saponin is opposite to that of digitalin; and Koehler has been led, on this account, to make a special investigation on the antagonism between these two substances (*Archiv für experimentelle Pathologie und Pharmakologie*, Bd 1, Hft 2, 1873, p. 138). His paper contains many facts of the greatest interest and importance, but we must content ourselves by briefly mentioning a few of the more prominent results of his experiments. The animals

employed were frogs, rabbits, and dogs. It was found that digitalin increases or reproduces the heart's contractions after they have been retarded or suspended by saponin, and that, likewise, saponin increases or reproduces the heart's contractions after they have been retarded or suspended by digitalin. Digitalin is able to prevent, for a considerable time, the paralysing action of saponin upon the regulator-nerves of the heart. It also prevents, for a somewhat longer time, the action of saponin in reducing blood-pressure and paralysing the respiratory centre; but it does not seem to antagonise the lowering of temperature. As might have been expected from the identity, or at least similarity, of some of their important actions, the antagonism between them is, however, incomplete. Thus, although digitalin can retard for some time the paralysing action of saponin upon the regulating nerves of the heart, it afterwards aids the latter substance in paralysing these nerves, and it joins with it in destroying the contractility of the cardiac muscle. The one substance cannot therefore act as an antidote to the other, and prevent death when a lethal dose of either has been administered.—(*Between Atropia and Physostigmia.*) Osler has made a number of observations to determine if any antagonism exists between the actions of atropia and physostigmia upon the colourless blood-corpuscles of newts, frogs, and human beings (*The Monthly Microscopic Journal*, August, 1873, p. 102). He employed one per cent. solutions of sulphate of atropia and sulphate of physostigmia, dissolved in half per cent. solution of common salt; and applied them in the proportion of four times as much reagent as blood in the case of newt's and frog's blood, and in the proportion of five to one in the case of human blood. The observations were made on a Stricker's stage, at a temperature of 39° Cent. Atropia caused all motion to cease in the corpuscles; sooner in the blood of the newt and frog than in that of man, and sooner with strong than with weak solutions. At the same time, the blood of frogs and newts poisoned by the internal administration of atropia showed normal amœboid movements, without any modification whatever. The action of physostigmia was found to be somewhat different. A solution of the strength of 1 to 800 of water did not modify the movements of the white corpuscles; but a solution of 1 to 300 greatly impeded the formation of processes, and caused the movements to be of an undulating and heaving character; while a stronger solution produced the same changes as atropia. One or two per cent. solutions of either substance acted distinctly on the red corpuscles, producing irregularities of surface from involutions and cuppings; but scarcely two corpuscles were affected alike. The result of applications of both substances was that no antagonism exists between atropia and physostigmia, so far as their topical action on blood-corpuscles is concerned. The special changes produced by each substance could be recognised in blood to which the two had been together added.

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ON SOME EFFECTS OF ALCOHOL ON WARM-BLOODED ANIMALS*. By C. Binz, M.D., *Professor of Pharmacology at the Bonn University.*

In the medical world alcohol is considered by some a *stimulant*, and by others a *narcotic*. Both designations are correct under certain circumstances; but they do not embrace the whole subject. A precise experimental analysis is necessary in order to obtain a definite view.

It seems to me that the question concerning the warmth of the body under the influence of alcohol is one of the most important. As far as I know it was first touched upon by Professor Nasse, in Marburg, 1846[1]; then again in 1848 by Duméril and Demarquay in Paris[2], and by Lichtenfels and Fröhlich, 1852, in Vienna[3]. They all had proved, in contradiction to the old and still much received opinion, that the warmth of the body slightly *decreases* after alcohol. These results were forgotten. Ringer and Rickards published, 1866, similar observations[4]; and Parkes and Wollowicz[5] refuted the former prejudice, which, by the way, Shakspeare represents very merrily[6]. But still, in the very last year, the old assertion of alcohol *elevating* the combustion of our body, with apparently great precision turned up again in Germany[7].

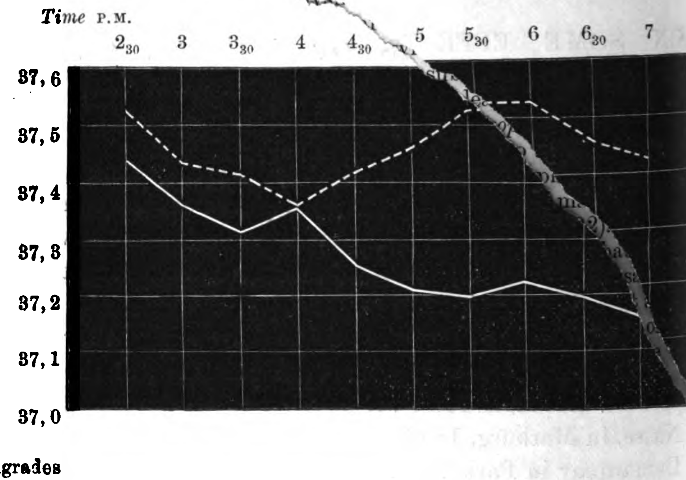
Whilst with us almost every one was convinced of the correctness of the view that using alcohol in fever would only feed the flames, I was reminded of the question by reading the accounts of Todd and his school. His communications could

* After a paper read in the Section of Biology of the British Association, Bradford, 1873.

not be right, if alcohol heats; his method must on the whole be called correct, if alcohol does not heat.

The experiments which one of my scholars, Dr Bouvier, made turned out as I expected[8]. They proved on healthy men and animals what the majority of authors have said since 1846. I print here a curve belonging to a *later* series of experiments, because all precautions in this case were observed with especial precision[9]. It is taken from a strong healthy man of twenty

I.



.... Normal curve, average of 7 observations.
——— Curve under the action of alcohol, average of 7 observations in the case of the same healthy subject.

years of age, who was suffering from a joint disease without inflammation, and who had abstained from all use of alcohol for a long time. Those hours were chosen as the time of measuring, in which, according to experience, the heat of the body never decreases by itself, in which, on the contrary, there is a tendency to an increase. The experiment is thus rendered more difficult, but in case of success it is made more evident. The person was measured seven days without, and in the meantime seven days after the administration of alcohol. The quantity of alcohol changing from thirty to ninety cubic centimeters (pure alcohol mixed only with tepid water and some sugar) was

always so regulated, that no trace of intoxication occurred. It is of course to be understood that all the outward circumstances during both series of days remained exactly the same. The comparison of each *single* curve of alcohol with the normal curves shows us clearly the difference. In order, however, to avoid every arbitrary explanation I have designed the average curve, and have drawn it in the graphic form.

My opinion, based on such experiments, may be summed up as follows: The pretended heat of the organism does not exist. The subjective impression is, at least partially, the consequence of an irritation of the nerves of the stomach and of the enlargement of the vessels arising in the skin. When given in small doses the thermometer shows no extraordinary increase or decrease of the temperature of the blood. Moderate doses, which lead by no means to drunkenness, show a distinct decrease of about half-an-hour's duration or more; and strong inebriating quantities evince a still more decided lowering of 3·5 to 5 F., which lasts several hours. The decrease in the temperature after moderate doses takes place most distinctly in warm-blooded animals, which have not for some time previously had alcohol administered. When inured to it, the organism does not answer to such doses by any measurable cooling or by the reverse*.

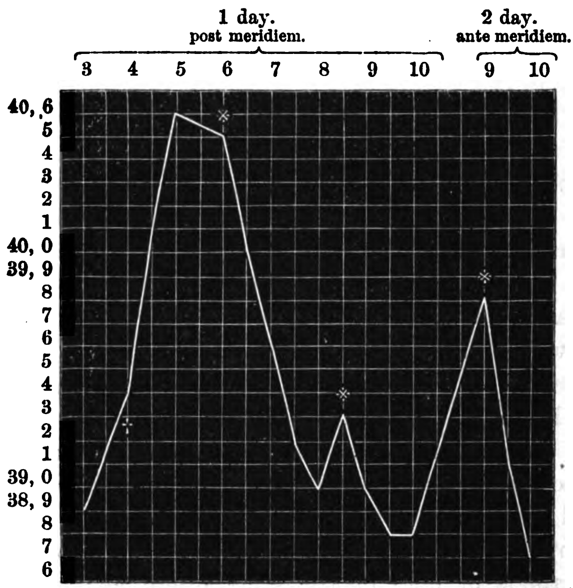
This is the result of more than one hundred measurings, which lasted generally from three to five hours. Only the rectum was used on these occasions, as the axilla is perfectly impracticable for ascertaining small variations. Another of my scholars has proved, that even the entrance of the thermometer into the contents of the rectum may entirely disturb the thermometrical result and especially obscure the obtained decrease.

Good results are yielded more easily by a feverish than by a healthy animal. I generally used for these experiments strong rabbits or dogs of the same origin and of the same quality, and injected under their skin some cubic centimeters of ichor or putrefying blood. As is well known, after thus pro-

* As to the great influence of habit I need only refer to tobacco. People who have been habitual smokers, and who smoke again occasionally, resist the toxic effects of the nicotine, etc., perfectly well. I speak here from my own experience.

ceeding, the warmth of the animal rises several degrees, and all the symptoms appear which are to be observed in human beings suffering from putrid fever. Particularly the severe symptoms in the intestinal canal remind us distinctly of our enteric typhoid fever. If the quality of the poisonous substance is right the animal expires in a few days. Not so, however, if simultaneously with the commencement of the experiment alcohol diluted with water is administered, either by means of the stomach or the skin. The temperature then remains lower from the beginning, the intestinal catarrh is slighter, the animal is more lively, and the one may be seen gradually to die, whilst the other takes its food kindly. The same effect takes place if we allow in both animals the fever

II.



A large dog.

At 3.30, Injection of 3 ccm. ichor under the skin;
6 o'clock, 5 ccm. alcohol (96 per cent.) with some water;
8.30, again 5 ccm. alcohol, and the following day
9 o'clock again 5 ccm.—The dog untouched the next day died.

(The injection of the ichor is marked by †, that of alcohol by *).

to make its way. I have drawn also the curve from such a case*. The analysis of this experiment shews me a threefold action produced by alcohol, (1) the diminution of the heat of the body, (2) reduction of the putrid processes, and (3) rising of the action of the heart. We here clearly see alcohol emerging from the small sphere allotted to it by many practitioners. It is much more than a simple stimulant in these circumstances, though it is no doubt sometimes a stimulant for the heart and the nerves.

By poisoning animals with ichor one can see clearly that even strong doses of alcohol need not be a narcotic. On the contrary, the ichor-poisoned animal which cowered down, sleepy and exhausted, becomes, after alcohol, lively, and runs about. Todd and his followers made similar observations in man.

Such experiments are easily repeated. It is only the question of procuring such ichor as will with certainty produce fever, two animals of the same quality, and then a careful injection of the pyretic matter; for it is clear, that if we go too far with the latter, the use of alcohol will not be able to save life. But in such cases it is always perceptible, even if the animal treated with alcohol dies, that there was some healing influence. The alcoholized animal either dies later or under less severe symptoms.

Space does not permit me to enter into any further details of these experiments. If we wish to gain the knowledge of their foundation and their rational use for life, it seemed suggestive to me, to search for the causes which lead us to the above-described results. It thus became a necessity to analyse the synthetic experiments in their separate parts.

What are the causes of the decrease of the temperature after the reception of alcohol? How does it work in the animal's economy? At first one would think an increase in heat *must* take place. Alcohol becomes easily oxydised. In the tissues of the animal body alcohol burns, if not given in excessive quantities, into carbonic acid and water, and thus warmth is set free. But the same occurs to a far greater extent if we consume grease or oil, and yet in this case no rise of the ther-

* For the sake of clearness I give only the curve of one animal, as it implies the other.

mometer is caused. The regulation of our whole system works in sufficient perfection to retain the bodily heat on the same scale. There exist other causes which counterbalance the little warmth in the circulation produced by the burning alcohol.

First of all one would search for these causes in the nervous system; but that which has hitherto been discussed under the name of thermic nerves, is still up to the present day entirely without proof, and also the centre for moderating the heat is doubted by many. If one separates the spinal cord of large animals at a certain height, one finds under some circumstances an excessive increase in the heat of the body, as Sir B. Brodie has first described in a man, who in consequence of an accident was thus situated. However the case may be, I have found in a series of experiments, that alcohol has the effect of lessening the temperature also of such feverish animals. Its influence, therefore, is independent of all those nerves which, having their issue in the brain, traverse the spine[9].

It can further be caused by the action on all the striped muscles of the body. According to Zuntz and Röhrig the muscles are the organs in which, with the assistance of the nerves, the greatest part of tissue metamorphosis, and especially oxydation, occurs. As one knows that after the reception of alcohol a feeling of relaxation in the striped muscles appears, so the connection between the two factors seems quite probable. How far it goes, further experiments must decide.

We have a clearer evidence of this from another point of view. The weaker the action of the heart the less quantity of blood is it able to throw to the periphery of the body, and therefore the cooling of the blood is diminished. Now, if we introduce, where the action of the heart is weak, an agent which drives more blood to the cool surface, the contrary occurs. The greater quantity of blood in the skin will irradiate a greater portion of warmth. In the same time the perceptible as well as the imperceptible perspiration becomes augmented. It is needless for me to explain, how, according to a simple physical law, a diminution of warmth must ensue.

Finally, one of the chief causes is the direct impediment of the activity of our cells. The great many microscopical elements of which our glands are composed, and through whose

action the albumen of food is decomposed, become slightly paralysed by alcohol. We have a very clear example of this fact in the action of alcohol upon yeast. It can be asserted from all points of view that the cells of the organism answer on the whole in the same manner to the reagents, as those of the mykoderma vini. The higher the percentage of alcohol in a fluid the less able is the protoplasm of the cells to work and to produce warmth. And not only does common yeast show us this, but in every other form of fermentation, particularly in the oxydation, commonly called putrefaction, the impeding influence on the protoplasm becomes most apparent; and even the highest and most complicated issue of the protoplasm—the hæmoglobin—is affected by it, when it is about to pass over its oxygen to other substances. It imparts its oxygen to combustible substances in a slower manner when but a small quantity of alcohol is present[10].

Of all these described effects which alcohol produces upon the animal organism each single one may be very slight, but summed up they mark a decided decrease in the thermometer. I will here only briefly say, that another series of experiments has proved to me, that also the post-mortem temperature is lowered by previous injection of alcohol[11]. As is known, this warmth after the death of almost every warm-blooded animal not only lasts for a shorter or longer time, but even rises by several degrees Fahrenheit. The fact, that this warmth also is under the influence of alcohol, proves to us the direct action on the chemical processes of the animal body more than anything else.

We thus see from all points that the pretended rise of temperature of the blood by means of alcohol exists as little as does the extraordinary cold in a fever patient whose bed is shaking under him. In both cases we are deceived by subjective impression, and only the instrumental observation gives us the truth.

It is *a priori* to be expected that alcohol will be not without influence on the metamorphosis of tissues. An agent that, consumed in somewhat larger doses, so clearly lowers the combustion, must also be supposed to decrease the urea and the carbonic acid, both the most important excretions of the organ-

ism. And in fact the researches of several authors prove to us that this is the case[12], although none of them has considered the great amount of CO_2, formed by the consumed alcohol itself. The gradual accumulation of fat in the tissues of drinkers is thus most probably explained, and also other occurrences are illustrated by these facts.

We feel in winter or in damp cold weather the want of consuming alcohol in small quantities, and we see further, that the abuse of it, especially in hot climates, leads to serious disturbances of the health.

Both of these occurrences can be explained by physiological facts. In a cold atmosphere our metamorphosis of tissues is accelerated, we consume more of them. This is agreed to by all physiologists, and proved by the experience of common life. The colder the climate the more substantial our food must be. Thus, when we imbibe a fluid which, without sensibly cooling our blood slowly, lowers the consumption of our tissues, we apply to the flaming furnace a kind of moderator. Exactly the contrary is to be remarked in India or in the Tropics. Here our tissue-metamorphosis is of itself sluggish. If we there continue to consume the same amount of alcohol, as we are in the habit of taking in any stimulating atmosphere, the consequence will be that to the already existing inertness of transformation in tissues and blood an artificial increase of this inertness will be added, which can only result in an accumulation of deleterious dross within our organism. The same, of course, is also observable when, in our moderate climates, we indulge freely in the use of alcohol; though greater quantities are admissible here.

With regard to the application of alcohol to fever patients we must not overlook, that its effect is not as strong and lasting as that of other antipyretic methods. Large and repeated doses are necessary to retain the lowering of the temperature. On the other hand, there are certain cases, for instance, where the heart is very weak, in which alcohol acts as an antipyretic, whilst quinine is powerless. Here alcohol revives the circulation, like subcutaneous injections of camphor, and thence arises the cooling at the periphery. Also camphor, as by me and one of my scholars has been proved, acts coolingly in putrid fevers[13].

In the treatment of all feverish illnesses, the first thing wanted is to suppress high temperatures. We thus remove the greatest danger, moderate the process of the disease, and give to the organism the possibility of resisting with success the internal cause of the illness. Further thermometrical observation is necessary in order to decide, in which cases alcohol may help to this effect, and in which other cases its administration may be useless or even injurious.

The question has been vehemently discussed, whether alcohol is a *food* or not. The answer depends quite upon circumstances. It is certain that we do not require alcohol under regular conditions to sustain life. Should, however, through any cause, such as cold air or feverish excitement, an increase of our tissue-metamorphosis arise, then the matter changes. Alcohol—according to the different basis—becomes a direct food; for burning without heating it yields to the body warmth and power of tension. It becomes an indirect food; for acting as we saw before, it moderates the wasting of the body.

As to the excretion of alcohol through the kidneys I agree, after repeated researches with Geisler's vaporimeter, thoroughly with those who declare, that under ordinary circumstances, especially in fever, only traces of alcohol appear in the urine.

In speaking of alcohol I mean the absolutely pure preparation of about 98 per cent., which I gave diluted with much water and a little sugar. All good alcoholic liquids, *the percentage of which one knows*, may serve for common use. But I protest decidedly against all those bad artificial and unpurified mixtures which are so generally given. The nauseous smell from the breath of the consumer indicates their injurious composition. Pure alcohol gives no taint to the breath, and good alcoholic liquids only leave that smell which belongs to their ethers.

Science has to solve the problem as to what alcohol can accomplish on the healthy and on the diseased frame[14], and in which form and way it ought to be administered. Such researches can be as little disturbed by the lamentable abuse of alcohol as by the somewhat immoderate reaction and agitation against that abuse.

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[13] J. Baum, in the *Centralblatt*, Berlin, 1870, p. 467; and his doctor's dissertation, Bonn, 1872.

[14] F. Riegel, Ueber den Einfluss des Alkohols auf die Körperwärme. *Archiv für klin. Med.* Bd. 12, p. 79 (August, 1873). An experimental paper with the same results as mine and those of my pupils.

ON THE ACTION OF INORGANIC SUBSTANCES WHEN INTRODUCED DIRECTLY INTO THE BLOOD. By James Blake, M.D., F.R.C.S., *San Francisco, California.*

The experiments I am about to describe will shew the action of the salts of Lime, Strontia, Baryta and Lead, when introduced directly into the blood of living animals. Since my last communication in connection with these researches the subject has been invested with additional interest from a discovery I have made of the connection between the atomic weight, and the physiological action of these salts of metallic bases. I find that *in the same isomorphous group* the intensity of physiological action increases as the atomic weight of the elements, shewing a new and important connection between physiological action and the molecular properties of these inorganic compounds. I have published a paper on this subject in the *American Journal of Science* for March, 1874. For the manner in which the following experiments were performed, I would refer to the May Number of the *Journal of Anatomy and Physiology*, for 1870.

Salts of Lime.

The experiment was made on a dog, weight 17 lbs. The manometer was connected with the arteries. A solution containing 4 grs. of chloride of calcium was injected into the jugular. Action of the heart slightly quickened; expiration performed by two or three spasmodic efforts; inspiration normal.—Inject 10 grs., no effect on the circulation; expiration more spasmodic. Inject 30 grs. In 12″ the action of the heart was quickened; the pressure in the arteries not affected except by the struggles of the animal; respiration spasmodic. Inject 60 grs. In 12″ the heart stopped beating: 30″, violent spasmodic respiratory efforts; head drawn down forcibly towards the chest; rigidity of abdominal muscles: 2′ after the injection, all movements ceased with the exception

of a strong quivering of the whole of the voluntary muscles which lasted 45″. On opening the thorax, auricles contracting, ventricles still: each cavity contained blood, right dark, left bright scarlet: blood coagulated firmly. The muscles that were exposed on opening the thorax contracted some time after death.

Exp. 2. Injection of nitrate of lime into the veins; animal not confined. Inject 3 grs. into the jugular; no appreciable effect. Inject 6 grs.; expression of pain as the injection was made. Inject 14 grs.; same expression of pain; expiration spasmodic: animal jumped about apparently from uncontrollable action of the muscles. Inject 14 grs.; 12″ after injection, efforts to vomit; animal seems weaker. Inject 14 grs.; animal jumped about with involuntary action of the muscles: 45″, lay down apparently from weakness. Inject 20 grs.; animal kicked about with irregular choreic movements; no expression of pain; after the general movements had stopped, the fore-legs were in continual motion. Inject 40 grs.; 10″, heart's action arrested: 40″, respiration stopped in a general tonic spasm; this relaxed in 1′ 30″, and three or four respiratory movements took place. On opening the thorax, the heart was still; left cavities contained scarlet blood[1].

Salts of Strontia.

Exp. 3. A solution containing 4 grs. of chloride of strontium was injected into the jugular: 10″, the pressure in the arteries diminished 1½ in.; 15″, pressure again at its former height 6 to

[1] In the last number of *this Journal* (p. 218) there is a notice of a communication made by Rabutreau to the Académie des Sciences, in which he states that the quantities of the salts of lime and of potash which, when introduced directly into the blood, destroy life, are about equal; and as the atomic weights of these substances are nearly the same he adduces this fact in support of the statement he had made as to the connection between the atomic weight of metals and the intensity of their physiological action. The result of my own experiments with these two classes of salts is, that they afford the most striking proof against the existence of such a connection as he has stated. In the many experiments I have performed with the salts of potash I have found them six or eight times as poisonous as the salts of lime, ten or twelve grains of nitrate of potash being sufficient to arrest the action of the heart, while it requires about one hundred and twenty grains of nitrate of lime to produce the same effect. It is only between members of *the same isomorphous group* that the intensity of physiological action increases with atomic weight.

8 in., oscillation greater, action of the heart slower. Inject 12 grs.: respiration affected in 10″, pressure diminished: 15″, pressure rising: 45″, 9 in., oscillations 3 to 3½ in., heart's action slower. Inject 20 grs.: 18″, the action of the heart arrested, respiration continued uninterruptedly a minute after the heart had stopped, it then became intermittent but full inspiratory movements took place three minutes after the arrest of the circulation. On opening the thorax the ventricles were still, nor did they contract on irritating them with the scalpel; auricles contracting rhythmically, and continued contracting for five minutes. Left cavities contained scarlet blood which coagulated firmly. Shortly after the thorax had been opened the whole of the muscles of the trunk began to contract, and contracted for more than a quarter of an hour. When the intercostals had been exposed in opening the thorax they contracted so as to move the ribs forty minutes after death, although the temperature of the room was 42°.

Exp. 4. Injection of chloride of strontium into the arteries. Pressure in arteries, 4 to 7 in. Inject 12 grs. of the salt into the axillary artery: 5″, action of the heart quickened, oscillation less: 45″, the action of the heart as before the injection. Inject 24 grs.: the same action on the heart 5″ after the injection; no other marked symptoms. Inject 36 grs.: immediate expression of pain. The direct effect on the heart could not be ascertained on account of the movements of the animal: 20″, it was stopped, probably earlier: 30″, respiration suspended for 1′ 30″; the muscles of trachea and larynx then began to contract, and in a few seconds the movement extended to the whole of the respiratory muscles; fourteen respiratory movements were made, they were short and spasmodic, lasting about 1′ 35″. On opening the thorax, the auricles were found contracting, ventricles still; left cavities contained scarlet blood. Four minutes after respiration had ceased the whole of the voluntary muscles commenced contracting so as to produce general movement of the body. The muscles of the leg contracted with sufficient force to move the body on the table when a *point d'appui* was furnished to the feet. These contractions continued more than a quarter of an hour.

Salts of Baryta.

Exp. 5. A solution containing 0·25 grs. of chloride of barium was injected into the jugular: 12″, the arterial pressure slightly increased: 1′, action of the heart slower. Inject 0·50 grs.: 10″, slight diminution of the pressure, heart's action fluttering: 14″, pressure increased 1 in. above the level before the injection. Inject 1 gr.: 11″, pressure diminishing with fluttering action of the heart: 14″, pressure again increased, heart's action slower and very irregular, two or three quick beats and then a number of slow ones. Inject 2 grs.: 12″, action of heart arrested; respiration continued irregular for 1′ 30″, it then became intermittent and ceased 2′ 45″ after the heart had stopped. On opening the thorax, the auricles were contracting vigorously, and continued contracting for some minutes. The ventricles were still, and did not contract when irritated. The left cavities contained scarlet blood. Five minutes after the thorax had been opened the muscles commenced contracting and continued in motion for fifteen minutes. When injected into the arteries, the salts of baryta are exactly analogous in their action to the salts of strontia. The quantity required to arrest the action of the heart is about 3 grs. When the salts of baryta are injected into the veins and the animal is left at liberty, no marked effects are produced until a sufficient quantity has been used to arrest the action of the heart.

Salts of Lead.

Exp. 6. Pressure on arteries 5·5 to 6 in. Inject a solution containing 3 grs. of acetate of lead into the jugular: 7″, pressure diminished: 11″, pressure increased to 7 à 7½ in. Inject 14 grs.: 7″, pressure diminishing: 11″, pressure 5 in.: 15″, pressure 3 à 10 in. Inject 30 grs.: 7″, pressure diminishing; 14″, pressure, 3 in., heart's action slow, oscillations 1 to 1½ in. at each pulsation; violent struggles 20″ after the injection.—Inject 60 grs.: 10″, the action of the heart apparently arrested, pressure fell to 1 in., no oscillations: 1′, respiration stopped. The animal lay as if dead for two minutes, the heart then commenced beating and respiration was renewed; the pressure in the arteries increased, so that in two minutes it was up to 8 in.—Inject 60 grs.: 10″,

heart stopped: 25″, respiration arrested in a violent spasm; when this relaxed the animal lay as if dead for 2′ 30″, when respiration again commenced and continued about 1′; the heart could also be felt pulsating, but no effect was produced on the manometer. On opening the thorax the right auricle only was contracting. The trachea and bronchial tubes were filled with frothy fluid tinged with blood; lungs oedematous. The frothy fluid had escaped from the mouth before death in considerable quantities. Shortly after the thorax had been opened muscular movements took place over the whole body, and one of the fore legs was moved strongly for many minutes.

Injection into Arteries.

Exp. 7. Pressure 5 to 6 in. A solution containing 6 grs. of acetate of lead was injected into the axillary artery: 4″, pressure rising: 30″, pressure 12 in. 1′: 15″, respiration arrested, pressure falling slowly; two minutes after respiration arrested, pressure 10 in.; three minutes pressure 6 in., heart's action regular, oscillations 0·5 in. At seven minutes after respiration had ceased the pressure had fallen to 2 in., heart's action weak. At this time one deep respiratory movement was made; its immediate effect was to increase the pressure to six inches. There was no other respiratory movement, and the action of the heart gradually became weaker as in asphyxia, apparently stopping in about three minutes. On opening the thorax the heart was found to be still contracting feebly, lungs natural.

The above experiments shew that the more strictly isomorphous members of this group Lime, Strontia and Baryta are essentially heart poisons, that is, they kill by arresting the action of the heart. Their apparently exclusive action on the heart is most evident when they are injected into the arteries, as then no marked effects are produced until they are introduced in such quantities as to reach the heart in a sufficiently concentrated state to destroy its irritability, the quantities required being much larger than when introduced into the veins. The action of these substances in paralysing the heart is owing to their direct contact with its tissues, although some action is exerted on the organ through the nervous system, as is shewn

by the change caused in its movement five or six seconds after they are injected into the arteries, and before they have had time to reach the coronary arteries. The action of the salts of Lead differs in many respects from that of the other members of the group. In its action on the lungs, and on the muscular coats of the arteries, it agrees with the salts of silver, with which substance it has certain isomorphous relations; but in the peculiar action on the voluntary muscles, which distinguishes the elements of the Baryta group from all other inorganic compounds with which I have experimented, the salts of lead agree with the other members of the group. This action on the muscles is most strongly marked after the injection of the salts of Strontia and Baryta, and shews itself sometimes in a curious manner. In one instance in an animal killed by chloride of strontium, there were no movements until ten minutes after death. The muscles of the ear then commenced contracting, so as to cause it to move; and from this point the muscular contractions spread until all the muscles of the trunk and limbs were in motion, and they continued contracting more than a quarter of an hour. The longest time after death in which I have observed these spontaneous contractions of muscles has been forty-five minutes; this was in the muscles of the penis and scrotum in a dog that had been killed by the injection of chloride of barium into the veins. It is probable that the occurrence of respiratory movements so long after apparent death (in one instance seven minutes) is connected with the action of these substances on the muscles, as the contractions they cause are not simply contractions of individual muscles, but coördinated movements. This action on the voluntary muscles is curiously contrasted with their action on the muscular tissue of the heart, at least on the ventricles, as I believe the irritability of the auricles is not affected, the action of the Baryta group on the heart being in this respect the reverse of the action of the chlorine group, which, as I have already shewn, destroy the irritability of the auricles, whilst increasing that of the ventricles. The salts of Lime, Strontia and Baryta exert but little action on the muscular coats of the arteries, either pulmonic or systemic; whilst the salts of Lead cause contraction in each set of vessels, agreeing in this respect with the salts of silver. I shall

not however at present discuss the connection of these facts with the general question of muscular irritability, or do more than allude to the pathological changes caused by lead in the muscular tissue, although they afford a new point of view from which to investigate these questions. I would state that my experiments prove that these muscular contractions are caused by the direct contact of the substance with the muscular fibre. The physiological action of these substances furnishes a well-marked example of the increase of the intensity of physiological action in the same isomorphous group as the atomic weight is greater. Thus we find:

	Atomic weight.	Quantity required to kill.
Lime[1]	40	100 grs.
Strontia	87	36 grs.
Baryta	137	4 grs.

With the salts of lead this connection between the atomic weight and intensity of physiological action does not exist, but, as has already been shewn, they also differ in their physiological action in many respects from the other members of the group. That this difference should be in the direction of assimilating their action to that of the salts of silver, with which they have isomorphous relations, is interesting, as when molecular physics shall be able to point out in what consist the differences and the resemblances of lead to the baryta and soda groups, we shall probably attain more definite notions as to the causes of the physiological phenomena to which it gives rise.

[1] The quantities of the different salts used in these experiments are undoubtedly greater than would have been required had they been injected at one time. My object has been to ascertain the general physiological action of the substances, and not the smallest quantity that would arrest the vital reactions. As, however, the experiments were all performed in the same manner the data they furnished are fairly comparable.

ON DOUBLE-BODIED MONSTERS, AND THE DEVELOPMENT OF THE TONGUE. By PROFESSOR CLELAND, M.D., F.R.S., *Galway*. (Pl. VIII. Figs. 3 and 4.)

SOME time ago I received from my colleague, Dr Browne, a kitten with four additional limbs attached to its breast (Fig. 3). Otherwise it appeared to be normal; but when I opened its mouth I was much surprised to find that it had a cleft palate, and that the well-developed tongue lay completely in the nose, supported by the incomplete halves of the palate. This is the point which is most remarkable; but as the monstrosity is one which to my mind presents a considerable degree of interest, and is less easily explained in detail than most others, I shall shortly describe the more important peculiarities of the specimen, before discussing further the position of the tongue.

The mother of the little monster, instead of biting the umbilical cord neatly away, appears to have been so discomposed as to have torn her unsightly offspring, and removed the whole intestine as far up as the middle of the stomach, and down as far as the rectum; but with this exception and the removal of part of the liver, there was little damage done.

The appended hinder-limbs lay in the middle line, while the fore pair inclined to the right side. The left appended fore-limb had the head of its humerus placed between two portions of the right sterno-mastoid of the supporting fœtus; and extending upwards from this point was a lamina of tissue passing inwards in front of the prevertebral muscles, and containing a cord which was attached superiorly to the base of the skull in the sphenoidal region, in the middle line, in intimate connexion with the back of the pharynx (Fig. 4, *q*). The anterior division of the right sterno-mastoid muscle was partly attached to the head of the humerus of the right appended fore-limb, and partly to the end of the cartilaginous left half of the divided sternum, while its posterior division was attached to the right half of the sternum. The scapulæ of the two appended fore-limbs were directed towards one another, with their dorsal

aspects toward the surface, and had their bases united together; and immediately beneath these scapulæ, united to them by fibrous tissue and muscle, was a pelvis reduced to the simplicity of the piscine form, composed of two bones, which met in the middle line and each supported on the superficial aspect the head of a femur. Thus the four appended limbs diverged from a central table formed by the union of scapulæ and pelvic bones. All of them were smaller than those which belonged to the developed kitten; and the left fore-limb had only three toes, while the others had the proper number.

The sternum, as has been said, was in two parts, which had not begun to ossify. These were separated one from the other by the table of origin of the appended limbs, and, toward the hinder part of each, a long pointed process projected inwards towards its fellow on the deep side of the table referred to (*s*, *s*). At first, I thought that these were the elongated xiphoid extremities of the half sterna; but a more careful examination showed that the xiphoid extremities were in their proper positions, and that these were separate elements. They are probably to be looked on as rudimentary half sterna belonging to the appended embryo.

A deeper dissection exhibited two tracheæ with one pharynx and œsophagus between them. The left trachea belonged to the developed kitten; the other was situated with the properly vertebral aspect foremost, that is to say, the incomplete parts of its rings looking forwards; and it was surmounted with a larynx smaller than the other, opening on the occipital wall of the pharynx, with its epiglottis next the skull, and without any tongue connected with it.

The thorax contained two completely separate hearts; and in connection with each was a well-developed pair of lungs; those, however, which lay in the middle of the cavity, between the two hearts, being more compressed than the others. The left heart was that of the developed kitten. It had two ventricles and two auricles: its aorta and pulmonary artery were normal, and its right auricle received an anterior and posterior vena cava normally. The left heart was slightly smaller; it had no pericardial sac, and had only one auricle and ventricle. Its auricle had two rudimentary appendages directed to the

vertebral aspect, and received a vein (*t*), formed by union of the azygos vein with an anterior trunk into which fell the left subclavian and a deep vein from the left side of the neck. The arterial trunk of this heart, springing from between the auricular appendages, formed an arch directed towards the left, and approaching the sternal wall of the thorax to be continued into a descending aorta which supplied the appended limbs and gave off a hypogastric artery; but before coming to the surface it communicated by a large branch with the anterior end of the descending aorta of the developed kitten.

On the visceral side of the appended pelvic bones was a little round body, apparently filled with meconium (*n*); and from this a structure extended to the damaged umbilicus. I believe that this was a vestige of intestine which had separated, near the umbilicus, from that of the developed kitten, and ended in a blind dilated rectum.

Concerning monsters consisting of a perfect fœtus, with an appendix which is "an acephalus with four extremities," Vrolik states that, in the instances described, genital organs existed in the appendix, but the anus was closed. Also, "in many cases the evacuation of urine has been observed; the appendix showed circulation of blood, it had its own temperature, and was dependent for nutrition on the chief or perfect body: in the interior were found uropoietic organs, vessels connected with those of the chief body, and an imperfect intestinal canal (Otto, Serres). In the supporting fœtus are sometimes found traces of double organs (Otto, Serres, Rosenstiel)[1]."

The nearest approach, which has come under my notice, to the monster now described, is one of those described by Serres[2], and alluded to by Vrolik. It also was a kitten with the appended extremities very similar to those in my case; but the hinder-limbs were about an inch and a half removed from the fore-limbs. The rectum of the appended fœtus was prolonged, after a very short course, up to a cæcum, whence an ileum was continued to join that of the developed kitten in its lower third. Thus, it will be perceived, an intestine, single above,

[1] Vrolik, article Teratology, in *Cyclopædia of Anatomy and Physiology.*
[2] Sur l'organisation anatomique des monstres hétéradelphes. Reprinted from the *Mémoires du Muséum.*

was double below, in the fashion common among double monsters which have duplicity of the lower part of the trunk. Further, in Serres' monster the appended fœtus had a urachus and a pair of kidneys; and the representation of these latter enables me to pronounce as a renal rudiment, in my specimen, a small mass of substance near the lower end of the additional aorta, which could not otherwise have been recognised. The fore and hinder appended extremities were united by a thread, which Serres describes as nervous and ending at its extremities in ganglia to which converged the nerves of the limbs. There were two pairs of lungs, and two tracheæ opening into a single gullet, exactly as in my specimen; but, curiously enough, there was one respect in which the appended fœtus was less developed than in my specimen; for in Serres' kitten there was only one heart. The artery of the appended fœtus came off from the aorta in front of the tracheæ, and turned to the right, while the arch of the aorta turned to the left.

Within my reach for consultation are the descriptions of two other monstrous kittens, in both of which there was a single head and two fully developed bodies. The first of these is by the second Monro[1]. In his specimen the cerebro-spinal axes were distinct; that of the right fœtus presented a complete encephalon, while the other had a much smaller "spherical body supplying the place of brain and cerebellum." Unfortunately no description is given of the osseous surroundings of this spherical body, and it is not apparent how it was related to the cranium. In this monster there was only one heart, from the left ventricle of which one aorta was sent out, which soon divided into right and left aortæ proper to each body. There were, however, two pairs of lungs; one pair connected with a normal trachea, and the other opening by separate bronchi into the lower part of the œsophagus. Another monstrous kitten has been described by Dr M^c^Intosh in the pages of this *Journal* (May, 1868). In this specimen bifurcation began in the back part of the skull, and there was a single cerebrum continued into a double cerebellum, pons and medulla. Two tracheæ were disposed in a manner evidently

[1] *Structure and Functions of the Nervous system*, 1783. Table XII.

exactly similar to the disposition in my specimen; and there were two distinct hearts, one much larger than the other, and supplying both sides of the head.

From a comparison of the different monstrosities referred to, it may, in the first place, be manifestly gathered that monsters with appended limbs have fundamentally the same origin as monsters with one head and two bodies. The nomenclature which speaks of the appended limbs as belonging to an "acephalus" is as misleading as could well be conceived. It is intended to convey the idea of a headless embryo being added to an otherwise normal fœtus; whereas the real state of matters is, that the head is the common property of the developed and the appended body. This is evident from two considerations: first, the resemblance of monsters with appended limbs to double-bodied monsters, and secondly, from double-bodied monsters being always the result of fissiparous division of a single embryo. In Dr McIntosh's specimen the fore-part of the head manifestly was the only undivided part of an embryo which had undergone fission from behind forwards at a period prior to the laying down of the primitive groove; and, if one of the bodies so produced had suffered arrest of development in the spinal region, a specimen like that of Serres or the one in my possession would have been the result.

Yet there is this to be said in favour of the term "acephalus" for the appended fœtus, that by far the most probable hypothesis to account for the production of completely separate acephali is that they have become, in process of early growth, detached by fissiparous division from the developed fœtus which always accompanies such a monstrosity, and are therefore to be considered as a third variety of result from posterior fission of an embryo. In this view the head of the developed fœtus is no doubt originally the common property of it and the acephalus, exactly as the head of a double-bodied monster is common to both bodies; but the acephalus may be said to have forfeited its claim to property in the head when it parted company completely from it. It would be of the highest interest, when opportunity occurs, to institute careful dissection of the body of the developed fœtus born along with a separate acephalus.

It may be further suggested that, while in one set of in-

stances of posterior fission, the two bodies resulting therefrom grow nearly equally, in the other set the degree of imperfection of the less developed body is dependent on the effects of being stretched out, set on the rack, by the more rapid growth of the other. It will doubtless be granted that the tough string extending, in my specimen, from the base of the skull to the fore-part of the appendix, was really the vestige of the spinal column of the appended embryo. Thus, taking the view suggested, it might be said that the spinal column of the less developed body was reduced to the condition of a string, by being attached at one end to the skull, while a part beyond was, by its connexions with the fore-limbs of the more developed body, pulled out at a rate more rapid than that at which it had been able to grow; and that the continued existence of the string prevented the visceral parts from being as well developed as in the specimen described by Serres. So also an early rupture of the connexion with the head, and with the body of the perfect fœtus, in the case of a completely separate acephalus, would give freedom for growth of the part separated. Reasoning in the same way, the continuity of the spinal column with the head is seen to be a reason why the column should be liable to suffer by stretching, while the limbs, appearing in disconnected portions of blastema, are only carried to unexpected positions, remaining uninjured and developing freely.

In my specimen the appendix was the right division of the original embryo, and had been carried round to the front by the growth of the right thoracic parietes of the left or developed division. In fact, in all double monsters the divisions of the embryo lay originally side by side, and therefore, even in those in which two bodies most completely face one another, there is a ventral aspect originally turned towards the yelk, to be distinguished from a dorsal aspect. In this connexion it is interesting to note that appendices, or so-called acephali, connected with the sacral end of the column, remain attached to the sacrum, while those adherent to the thorax are carried to the front; and it is not quite obvious whether this is to be explained by supposing that sacral appendices result from a later and less perfect fission, or whether the earlier and more perfect growth of thoracic parietes enables them better to carry to the front

appendices connected with them. I should incline to the first supposition.

The presence of two pairs of lungs in all the four instances of monstrosity recited, with formation of two tracheæ in three out of the four, while, on the other hand, in two cases there were two hearts, and in the other two only one heart, is very remarkable, and is even more so when it is considered that one of the bicardial monsters was double-bodied, and that the other, the one which had the smaller heart most nearly approaching in size to the larger, was the kitten in my possession, which had the other parts belonging to the appended body less developed than they were in Serres' specimen.

In trying to discover the explanation of these phenomena one is led to consider the mode in which fissiparous division in the embryo takes place. We have no evidence that complete fission of an impregnated ovum prior to the appearance of the embryo ever occurs; and the phenomenon which has been met with, and which explains the formation of double monstrosities, is fission more or less perfect, not of the ovum, but of the embryo placed thereon. The embryo and the ovum are two very different things; indeed to my mind it is obvious, as I have stated elsewhere[1], that the development of the embryo from the ovum is a process of gemmation; and, especially when multiplication of the embryo by division is attempted, I can see no difference between the plan of reproduction in vertebrata and the alternate generation of medusæ. But what interests us at present is that fissiparous division, when it occurs, being confined to the embryo, the remainder of the ovum presents the obstacle which most interferes with its completion, and the part most intimately connected with the yelk or cavity of the ovum exhibits least tendency to divide. That part is the alimentary tube, particularly the portion neighbouring to the umbilical vesicle. Indeed the tract of intestine usually found single in double monsters extends from the duodenum to the place of connexion with the umbilical vesicle; but the reason why it reaches so far up I judge to be connected with the very early appearance of the liver; having found in a dissection of a

[1] *Animal Physiology*, p. 276. 1874.

double monster, made many years ago for Sir James Simpson, and still in the Edinburgh University Museum, a common duct, from two pancreases, and a liver with two gall-bladders, opening at the angle of junction of the two duodena.

We see, then, that division of the embryo by fission, whether extending from before backwards or behind forwards, meets with least obstruction on the dorsal aspect; and the things which strike me as the interesting points to be learned from the duplicity of the air-passages and occasional duplicity of the heart in double-bodied monsters with single head are, first, that in a part already separated from the ovum, the tendency to duplicity exhibits itself on the ventral as well as the dorsal aspect, while the gullet situated in the centre between the two aspects remains single. Secondly, it is interesting to note that while the tendency to duplicity is interrupted on the ventral aspect by the connexion with the yelk, the direction in which the duplicity has travelled continues the same in front of the interruption as behind, the fore part of the head being in every case single. That the heart is in some cases single, when the trachea, which must be considered as a deeper structure, is double, is perhaps to be explained by the earliness of the heart's first appearance; it may in one instance be already laid down as a single tube, and in another be not yet begun to form, at the time when the tendency to fission has travelled so far forwards on the ventral aspect.

The still earlier period at which the gullet appears may also be taken into consideration in connexion with the singleness of that structure. But I confess that it is a subject of marvel to me to find, in my own specimen, as seems also to have been the case in that described by Dr M^cIntosh, the right trachea, the smaller of the two, turned completely round, and the larynx opening on the cranial side of the pharynx. That the right heart was so turned is easily understood: it was carried round with the body to which it belonged. But applying this explanation to the larynx and trachea, the axis of revolution must be considered as passing along the centre of the gullet; and it is simply impossible to conceive a larynx and trachea, originally placed to the right, rotating to the left, and continuing their revolution till the larynx touched the vertebral column of the body to which

it was opposed, and the trachea regained its position on the right side. Therefore, either the larynx revolved toward the right side of the gullet after the body to which it belonged had revolved from right to left, which seems impossible, or the revolution of the body took place before the windpipe began to be developed. Even on that supposition it might well puzzle a metaphysician to apportion the parts of the gullet between the bodies to which the windpipes belong. Explain it as we may, there seems to be an interlocking of the areæ in which the formative forces of the two bodies dominated. Matters are not made simpler by the consideration that in my specimen the vestiges of the spinal cord of the right body were adherent to the top of the gullet, close to where the larynx opened.

These remarks have become much fuller than I had intended. Let us revert now to the peculiar position of the tongue in the cavity of the nose, folded round the vomer, and above the cleft palate. In all the three cases which I have cited to illustrate the kitten in my possession the palate was cleft, and apparently so widely, that nose and mouth made one cavity, and no information was to be gained as to the relation of tongue to palate. But the circumstance which led me to think of writing this paper was, that a few weeks ago my colleague, Dr Doherty, placed in my hands a spirit preparation of a two-headed kitten, belonging to the Montgomery Museum, and, finding it necessary to change the spirit, I took the opportunity to examine the mouths of the two heads of the monster, and found a very remarkable state of matters. In the right head the palate was cleft, and the tongue folded round the vomer and placed completely in the nose, exactly as in the double-bodied kitten. In the left head the palate was perfect, but the mouth contained no tongue; a pillar of substance, however, ascended in the back of the throat, completely filling the pharynx, so that at once it could be perceived without dissection that, in this head also, the tongue was impacted in the nose. The question however occurred, how is the tongue disposed? With the complete development of the palate there is ordinarily associated complete formation of the nasal septum; but in this instance there must needs be either deficient development of the septum, or deficient development of the tongue. To determine this point

I obtained permission from Dr Doherty, and laid open the nasal cavity on one side, so as to display the relations of parts. It was then found that the tongue was less than half its proper size; the vomer failed to pass so far backwards as it ought, and the tongue was rendered bifid at its extremity, by its attempt to grow forwards into the nasal fossæ on each side of the vomer.

Let us explain the matter as we may, it is certainly very remarkable that, in six heads of monstrous kittens, the only examples of monstrosity in those animals coming, by observation or otherwise, under my notice, there should not be a normally developed mouth; that in five out of the six the palate should be cleft, and that in the three cases in which the palate existed, in whole or in part, the tongue lay above it. Nor does the matter seem less curious when it is considered that two of the heads were attached to a single body, while the other four were each in connexion with two bodies more or less completely developed. In all instances, however, the connexion of double structures with single was calculated to lead to crowding; and it is possible to imagine that this crowding led to the tongue being forced unduly upwards, and that the altered position of the tongue occasioned the cleft palate by interfering with the closure of that structure.

This displacement of the tongue is particularly interesting as throwing light on a question of development which has not, so far as I know, been settled by direct observation. The mouth, as is well known, makes its appearance originally as a depression of the integument; and afterwards, at a very early period, a cleft of communication takes place between this depression and the anterior extremity of the alimentary tube. The point to be determined is, whether the tongue is to be considered as connected with the integumentary depression, or with the mucous membrane. These monstrosities would seem to show that it belongs to the cul-de-sac of mucous membrane. The original roof of the integumentary depression is, indeed, a totally different thing from the ledges which, long after its rupture, appear and unite to form the palate; it is of similar nature to the membranes which originally occlude all the cervical clefts; and, as I have said, the position of the tongue above or below the palate might well depend on the amount of space left by the

growth of parts below. But while the normal tongue may be said to arise from the floor of the mouth forwards to the symphysis of the jaw, the arrangement in these cases of monstrosity shows that the root of the tongue makes its appearance first behind and afterwards grows forwards.

EXPLANATION OF PLATE.

Fig. 3. Kitten with eight limbs described in the text; somewhat reduced. Fig. 4. Dissection of the same; somewhat enlarged. *a*, cleft palate; *b*, tongue drawn aside from its position, grooved so as to fit round the vomer; *c*, *c*, lower jaw cut across; *d*, *e*, left and right larynx, surmounting their tracheæ; *f*, *g*, anterior and posterior vena cava of left heart; *h*, left heart; *i*, aorta from the same; *k*, œsophagus; *l*, aorta of right heart; *m*, hypogastric branch of the same; *n*, rectum of appendix; *o*, kidney of the same; *p*, right heart; *q*, cord, cut across, which attached appendix to base of skull; *r*, sterno-mastoid muscle; *s*, *s*, appendices to the two halves of the sternum; *t*, vein entering right heart; *u*, symphysis of the pelvis of the appended limbs.

ON THE CARTILAGES AND SYNOVIAL MEMBRANES OF THE JOINTS. By CARL REŸHER, M.D., *Privat-docent of Surgery in the University of Dorpat.* (Plate IX.)

IN a previous publication[1] I have given the results of an experimental study, which I had undertaken, of the changes which the joints undergo under conditions of prolonged inactivity. It was there shewn that, both in the young and in the adult, beyond those parts of the articular surfaces which are habitually in contact, the large, flattened, round cartilage-cells pass, by the gradual acquisition of cell-processes, into the spindle-shaped or stellate connective tissue corpuscles of the synovial membrane. In the same communication also I drew attention to the great similarity observable between this transition and that seen in the development of the so-called marginal zone of the synovial membrane, or, in other words, the portion of that membrane which extends over those parts of the articular surfaces which are not ordinarily in contact, and started the question as to whether this marginal zone were really to be regarded as a growth from and production of the synovial membrane or not. The generally received opinion is, that at the time when the cartilages corresponding to the several bones that are to be formed become separated from one another by fissuring of the cartilaginous substance accompanied by increased growth of the cells and liquefaction of the intercellular substance, at the same time a corresponding portion of perichondrium developes into the capsule of the joint; and finally, that those parts of the cartilaginous ends which are not constantly subjected to mutual contact become covered with an ingrowth of the synovial membrane. Hüter, in accordance with these views, conceives the marginal zone of the synovial membrane to be derived, not from the rapidly growing layer of embryonic cells by the dehiscence of which the cavity of the joint is formed, but, as a true ingrowth of the synovial membrane considered as a tissue the matrix of which is situated in

[1] *Deutsch. Zeitsch. f. Chirurgie*, Bd. III. p. 189.

the innermost layer of the capsule, and from which it becomes infolded over those surfaces of the joint which are not habitually in contact. If this opinion be correct, immediately after the formation of the joint each cartilaginous end should possess the peculiar form seen in the future bone, *e.g.* in the hip, the one the concave form, the other the convex: the latter (the head) will then have portions of the surface in constant contact or not, according to the movement which is permitted to the limb *in utero*. This, however, is not in accordance with our experience; on the contrary, we are forced to consider that at first the joints are very simple and incompletely developed, possessing gliding and rotatory movements around one axis only; and it is only in the course of development, by aid of the constant attempts which are continually being made to move the limb around other axes, that the stereometric forms of the articular ends, which determine the typical movements of the limbs after birth, become developed. In accordance, also, with the manner in which subsequent movements take place, marked changes are found to occur[1].

Whichever view of the formation of the articular surfaces is the correct one the question of the origin of the "marginal zone" of the synovial membrane still remains open. The investigation of fœtal joints can alone definitely solve the problem: the results of observations on these form the basis of the present article.

The investigation of the surfaces of embryonic joints presents considerable difficulty, the parts composing them being too small and soft to enable good sections to be made: the latter also are difficult to obtain on account of the sharp curves of the surfaces. For this reason I have not been able to subject the earliest stages to the microscope, but have been obliged to be contented with such as are to be found in embryo sheep of about two inches long; I hope, however, that the facts arrived at will appear sufficient to yield a correct notion of the development of the so-called "synovial processes[2]." I have carried on

[1] In illustration of the same principle I need only refer to the physiological changes which occur in the bones (particularly in the head of the astragalus) in the different forms of club-foot.

[2] The synovial ingrowths or processes referred to in the text are not to be confounded with the well-known Haversian fringes, which project freely in the

similar investigations in the embryos of guinea-pigs and dogs, and in new-born puppies, rabbits, and cats; besides studying the structure of the parts in full-grown sheep, oxen, and dogs, and in the human joints at different ages. In order to demonstrate the structure of these synovial processes, I have employed, as was done by Hüter, the method of treatment with nitrate of silver.

Before, however, the observations are considered in detail, it may not be out of place to say a few words as to the value of the silver treatment in the investigation of the synovial membrane. As is well known, the usual interpretation of the images obtained by means of the silver treatment has been called in question by Schweigger-Seidel, and doubt has been thrown upon the cellular nature of the figures and appearances which are produced in the synovial membrane by means of this reagent. His objections have been fully disproved as regards other organs (*e.g.* the cornea), in which, with different methods of treatment, corresponding outlines are always obtainable. Whilst firmly convinced that the same holds good with regard to the synovial membrane (having looked upon it in this light in the work which I have before referred to), it must be remembered that so long as neither the treatment with chloride of gold nor combined methods had been employed in the investigation of this tissue, a gap was left in the evidence as to the nature of the silver outlines. I anticipated that these would render service, more especially in bringing to light the exact meaning of the large white stellate fields[1], apparently belonging to the same category as those demonstrable in the cornea, but as to which it was uncertain whether they belonged to groups of cells or only to single ones. My investigations with respect to this point have been principally made on the joints of full-grown sheep and oxen, the tarso-metatarsal joints of which, and especially of the last-named, yield marginal zones a finger's-breadth wide. The sections were always made subsequently to the occurrence of the silver precipitation; in this way the clearest images are obtained, and there is no fear of cutting sections of cartilage from which the marginal zone has been accidentally rubbed off.

I had so often attempted to combine the staining with other reagents, such as carmine and aniline, with that obtained by the silver method, but without any great measure of success, that I was extremely pleased to find that hæmatoxylin, which I made trial of at Prof. Sanderson's suggestion, furnished a perfectly reliable means of

articular cavity, and the microscopic structure of which has been carefully described by Rainey and others, but are, as already mentioned, merely those portions of the synovial membrane which lie over the borders of the cartilage, and the connective-tissue corpuscles of which pass by gradual transition into the cartilage-cells.

[1] Comp. Hüter, *Virch. Arch.* XXXVI. Plate II. fig. 10.

staining the cell-nuclei. By the employment of sections which are sufficiently thin to obviate any sources of fallacy arising from the presence of the nuclei of the more deeply-seated cartilage-cells, it is not difficult to be convinced that the white fields on the brown ground of the silver preparation, from the more circular spaces of the cartilage to the stellate and epithelioid forms of the inner layer of the capsule, each contain either one or several (violet-coloured) nuclei. By this method then it is demonstrable that in each of the white fields of the silver preparation there lie, according to the size of the fields, one or several cellular elements. It is, however, impossible by this means to say whether the cells entirely fill the cavities, and by means of their processes extend into the lymphatic canaliculi, forming a complete anastomosing network, or not; for the elucidation of these points the treatment with chloride of gold is necessary. Without going farther into the description, I need only recommend a comparison of Figs. 3 and 4, which are taken from two preparations of the tarso-metatarsal joint of the ox, the one representing a preparation treated by the combined silver and hæmatoxylin method, the other with gold. In both kinds of preparations appearances are to be met with, which, in general form and in mode of branching of their processes, are more or less similar.

As the hæmatoxylin shews the presence in the silver preparations of cell-nuclei corresponding to the white spaces, so the treatment with gold shews that these nuclei belong to protoplasmic bodies, which—the conclusion will hardly be assailed—correspond on the whole to the spaces in the silver preparations. It is quite another question whether these masses of protoplasm completely fill the spaces or not. Proof of this could only be obtained were it possible to produce both the gold and the silver appearances in the same preparation; as is well known, however, if a preparation be treated first with silver then with gold, the only effect is to produce a reduction of the latter in the parts impregnated with silver, whilst the converse mode of treatment altogether fails to yield the silver spaces. The question must, therefore, so far remain unsettled. All one can say is, that in both silver and gold preparations appearances are frequently obtained, which, as regards form, are precisely similar, apparently even to the minutest details, although it is not everywhere possible to trace the same exact resemblance. For instance, the protoplasmic masses of the one might be said to be smaller relatively than the spaces of the other: the sizes, however, in both are so varied that it is difficult to compare them. If the forms obtained by the gold treatment differ from those obtained by the silver treatment, in one point more than in another, it is in the diameter of the processes, which here and there appear somewhat smaller and more tapering than those proceeding from the spaces of the silver preparation. On the other hand, the appearances presented in silver preparations, which have been placed in spirit, are in favour of the idea that the spaces are completely filled by protoplasm. In these it may here and there be seen, although, it must be admitted, not as a rule, that both the nucleus and the protoplasm are to be made out,

the latter appearing as a finely granular substance, which is separated from the brown intercellular substance by a crescentic, clear zone or space (perhaps caused by a shrinking of the protoplasm).

We may conclude, therefore, that the white spaces and canaliculi shewn to exist in the synovial membrane by treatment with nitrate of silver, correspond generally to a network of protoplasm (made evident by chloride of gold) consisting of connective-tissue corpuscles.

A similar statement may be made with regard to the cartilage-cells of the surface, which appear after treatment with silver as round white spaces, in which hæmatoxylin brings the nuclei into view, whilst on the other hand chloride of gold colours the protoplasm of the cells.

The appearances produced by the silver method having been thus explained, we may proceed by its aid to investigate the structure of the joints. If the head of the femur of a sheep's embryo 1¾ inch long be brought under observation, it is easy to satisfy oneself that the layer of cells described as an epithelium by Todd and Bowman, and by Reichert, does not exist. The appearances presented by the surface of the cartilage after treatment by this method are precisely similar to those seen within its substance. The cells at this early stage lie so close to one another, that there is no matrix apparent, and it is difficult even to make out their outline. All that is visible is an apparently homogeneous substance with nuclei imbedded in it of varying form, which might easily be taken for the cells themselves imbedded in an intercellular substance. Teazed preparations shew, however, that they are really nuclei surrounded with a variable amount of protoplasm. This observation coincides in the main with that made by Toynbee, who found (p. 163) that in the calf fœtus of 12 lines long there was no difference between the cells on the surface and those in the substance of the cartilage. The small processes which Luschka describes as projecting into the articular cavity I have not succeeded in coming across, either in these young ones or in older embryos: I have however only investigated the larger joints. The appearances seen in sections from embryos a little more advanced (sheep's embryo about 2½ inches long) are somewhat different. Besides places in which the above described condition obtains, others are also found in which the nuclei on the surface are rather more separated, brown lines appearing here and there between them. In some places the

irregular cartilage-cells are still closely juxtaposed, whilst in others they are somewhat larger and flatter, and fine brown lines shoot out between them, and in some parts completely bound the cells. Later on, the brown lines become more frequent and broader, and in certain places form an elegant network like that on the surfaces of the serous membranes, differing only in the fact that the territories marked off are commonly smaller and more irregular: the appearance is quite that of an epithelium (fig. 1, *a*). In older embryos this epithelioid appearance is still more common and complete, so that the larger part of the articular surfaces is covered with these flat epithelium-like cells. This is particularly well seen in the head and condyles of the femur of new-born rabbits and cats, and it is doubtless this that Todd and Bowman refer to in stating that (p. 127) "in the fœtus it (the epithelium) is continued over the whole cartilage." In this, however, I cannot concur, for the transition is gradual from the parenchymatous condition (without matrix) of the surface-cartilage to the appearance of a slight amount of intercellular substance, the latter becoming gradually more and more increased in amount, so that the white fields become separated from one another by broad bands of matrix, and assume an irregularly angular form; in fact, the appearances obtained are quite similar to those seen on the so-called "synovial process" of the adult[1]. In preparations taken from the shoulder-joint the surface-cells are roundish and connected here and there to one another; in those from the patella, more spindle-shaped and trailing off into processes (fig. 2, *e, f*), their long axis being parallel with that of the limb. These differences appear to me to be produced mechanically. The all-round movements of the shoulder-joint would naturally not influence the growth of the cells in one direction more than in another, whilst the single to-and-fro movement of the knee-joint might be conceived to have the effect of producing elongation of them. But be that as it may, it is certain that with the growth of the cartilage the intercellular substance,

[1] These forms of cells have been termed by Hüter *epithelioid* and *keratoid*, meaning thereby to imply a resemblance in form to pavement epithelium and to the connective tissue corpuscles of the cornea respectively. The term "keratoid" is however somewhat ambiguous, and has therefore not been adopted in this paper.

as well on the surface as in its depth, increases in amount and causes the cells to become more separated from one another.

To recapitulate:—The surface-cells in the earliest stages lie close together without intercellular substance; later on, the latter becomes developed as fine lines between the cells, producing an epithelium-like appearance (fig. 1, *a*); still later, by a further development of intercellular substance, the cells become more separated and irregular (as in fig. 1, *b*). The intercellular substance (matrix) becomes increased over, as well as between, the cells, so that in the adult there is a distinct layer of hyaline matrix covering the surface of the cartilage. While this is occurring the irregular stellate and angular cells on the one hand, and the elongated cells with trailing processes (fig. 2, *f*) on the other hand, become gradually transformed, in parts where the articular surfaces are constantly in contact, with loss of their processes into the round scattered cells of ordinary cartilage (fig. 2, *d*).

Thus much having been said as to the development of the parts of the surface which are constantly in contact, we have next to consider the condition of the articular surface in the neighbourhood of the capsule. At the time that the proper articular surface has the epithelium-like appearance before described, the same can be traced over its margin as far as the insertion of the capsule. The parts in which, in the adult, vessels and irregularly disposed cells are to be found, are covered in the fœtus merely by an epithelioid layer of cells similar, as just remarked, to that on the remainder of the surface of the cartilage; and it is only in places which are more affected by the movements of the joint that other forms are to be found. In size the cells are quite similar to those covering the cartilage, but differ from them somewhat in shape, being less polygonal and separated by rather more intercellular substance, into which they send processes which are, however, very short and knobbed, not long and tapering, and extending far into the intercellular substance, as is the case in the adult (figs. 3, 4). Fig. 1 is a correct representation of a portion of the peripheral surface of the glenoid cavity of the shoulder-joint from a sheep's embryo five inches long. The larger part of the surface presented a beautiful epithelioid appearance. In the figure, which corresponds

to the outer raised margin bounding the glenoid hollow, two parts are to be discerned, a vascular (*c*) and a non-vascular (*a, b*). The last-named must necessarily have been in contact with the head of the humerus, whilst the vascular part corresponds with the line of insertion of the capsule, and represents the *circulus articuli vasculosus* of W. Hunter. On the glenoid part (fig. 1, *a*) we see the regular epithelioid arrangement, on the synovial part (*c*) the irregularly disposed cells of the inner layer of the capsule, and between these every transition. The appearances seen give the impression of a change being produced in the previously more regular cells by the tension to which the synovial membrane is subjected in the neighbourhood of its insertion. In the knee-joint we find the different stages we have described still earlier. In sections from the surface of the patella of an embryo sheep, four inches long, comprising both the cartilage surface and the synovial margin, I have been able to see in silvered preparations how the cartilage-cells, as we approach the synovial membrane, begin to exhibit processes which get continually more numerous, and the cells more irregular, until they take on an appearance similar to those on the inner layer of the capsule, and indeed come into connection with them by freely anastomosing branches. Part of such a preparation is represented in fig. 2. At (*d*) the round white fields corresponding to the cartilage-cells are to be seen; at (*e*) these possess processes, at (*f*) they come to be still more branched and irregular. This part corresponds to the beginning of the capsule, the inner layer of which is to be seen in another part of the same preparation as a beautiful layer of epithelioid spaces.

Whilst in fig. 1 the cells of the cartilage-surface (*a*) still present an epithelioid arrangement, and those of the inner layer of the capsule may also be termed epithelium-like, in a further stage of development the appearances are quite the reverse. Over the cartilage the cells (fig. 2, *e*) are not only far apart, but present most irregular forms, whilst in the neighbourhood of, and upon the capsule they may be termed epithelioid. These differences get more and more marked as development proceeds. In full-grown animals the round spaces become so common, and the irregular ones so rare, that it is now no longer easy to find the latter, and it is only in particular localities that they

present themselves, and then in no great amount. Instances of such places are to be found in man at the lower border of the patella and the anterior border of the tibial surface of the astragalus (Hüter). Larger animals would naturally be expected to shew this more markedly. In accordance with this in oxen, in sections of the articular surface, including the marginal zone, which had been treated with nitrate of silver, and subsequently with hæmatoxylin, I have observed almost every transition, from the irregularly stellate cells, like those found in the cornea, to the round cell-spaces of the cartilage.

It rarely but occasionally happens that a network of irregular cells is seen in the immediate neighbourhood of round ones: here also, however, some of the cartilage-cells are met with, possessing a single process and representing rudimentary stellate cells, as pointed out already by Hüter and Böhm. The meaning of these silver images needs but little further explanation. Assuming the white fields to represent masses of protoplasm with nuclei, and the round ones in the homogeneous ground-substance to correspond with the cartilage-cells, the rest, whether possessing processes or arranged like an epithelium, must be looked upon as connective-tissue corpuscles.

The Synovial Membrane.—In spite of the difficulties presented by the softness of the embryonic tissue to subjecting silvered portions of the synovial membrane uninjured to the microscope, I have nevertheless been so far successful, and I am able to confirm, in every respect, in the embryonic tissue, the observations of Hüter upon the adult; and I must distinctly deny the existence of an epithelium, or at least of a distinct layer of cells, such as Landzert and Albert have lately described as covering the vessels and the sublying cell-spaces. It is true that places are to be found on the inner surface of the capsule in which the cells are regularly arranged, like those upon the surface of the cartilage, represented in fig. 1, *a*: such patches are, however, never very extensive, and always shew at their margins a transition to the more separated cell-spaces. The layer is in reality characterised by the variety of the cell-forms, and the differences in width and regularity of the intercellular substance; and it is only upon the surface of the cartilage itself that, as before seen, any appearance similar to that of the serous

epithelium is observed: as we approach the capsule this disappears, and the irregular arrangement is seen (fig. 1, *b, c, c'*).

As regards the relations of the vessels, also, I can confirm Hüter. In the same manner as in the marginal zone they lie between the epithelioid cells, so here on the inner surface of the capsule. It is true that they are very often seen covered by cell-spaces (*Saftcanälchen*); some, however, rise to the surface.

As we have seen then, in the earliest stages of fœtal life the form and arrangement of the cells on the surface of the cartilage are precisely similar to those in its substance. They then become flattened, and form a layer of cells somewhat similar to a serous epithelium. At the borders of the cartilage these become jagged, and pass into forms which are more and more like those of the "Saftcanälchen" of the serous membranes. This arrangement obtains throughout life on the inner surface of the capsule. A change, however, takes place in the cells on the surface of the cartilage, and this change depends, firstly, on the growth of the articular surfaces; secondly, on the varying conditions of contact and pressure to which they are exposed.

With respect to the first cause it may be remarked that the cells become more or less evenly or unevenly disposed, according as the general articular surface becomes evenly developed or not. Thus on the concave articular surfaces, the growth of which is more or less uniform both near the centre and at the periphery, the cells, whether epithelioid, stellate or rounded, are more or less similar throughout both in form and disposition, whereas on the convex surfaces such as the head of the humerus or femur, the superficial cells in the parts in the neighbourhood of the neck are far less separated than those nearer the centre of the articular surface, where growth and development are more rapid.

With regard to the second, it is to be noticed that in parts of the surface which are always in contact, the epithelioid cells become, as development proceeds, irregularly stellate, finally losing their processes and becoming round, so that by the time of birth the epithelioid arrangement has in most animals disappeared at these parts.

The fact of the conversion of the epithelioid layer on the articular surface of the fœtal cartilages into the large and round

cartilage-cells, met with on the surfaces of joints which are no longer embryonic, such conversion occurring not only on parts of the surface which are not constantly in contact, but also, although at an earlier stage, on parts which are, has not, I believe, been previously pointed out.

One is at first inclined to ascribe this conversion of the epithelioid cells into stellate corpuscles, and those again into round cartilage cells, to the rubbing together of the articular surfaces. I am, however, rather disposed to believe most of it to be due to the mutual pressure of the opposed cartilages, and this I am led to think because new canal-systems become formed below the superficial network of protoplasm (fig. 4), long processes passing down from this into the substance of the cartilage, where, of course, there is no exposure to friction. Such a one is seen in fig. 3, *g*, the body of the cell being quite on the surface, the process, as may be seen by focussing the microscope, dipping down into the matrix.

As the pressure and counter-pressure exercised on the different parts of the articular surfaces vary with the direction and frequency of the movements of the joint, so we find corresponding differences in the cells, such as have already been more than once alluded to. The converse fact I had previously demonstrated experimentally in the most striking manner by keeping the joints of dogs at rest. Under circumstances such as these, in which all the effects of pressure and movement are removed, the cells on the articular surfaces again take a more or less epithelioid arrangement. This change is accompanied by an absorption of intercellular substance, and extends also to the deeper layers of the cartilage.

These facts shew, I think, that the synovial process, so called, is not to be looked upon as an ingrowth of the synovial membrane, as some have asserted, but rather as being formed *in situ* as the development of the joint proceeds, its cells being intimately related both by the history of their development and by the presence of intermediate forms with the cartilage-cells of the articular surface. Whether it is derived from the dehiscing substance or not I am unable to say, not being sufficiently familiar with the changes which the newly-formed cells here undergo; but this at least is certain that, at a later period,

both the cells on the inner surface of the capsule and those on the surface of the cartilage form a continuous layer, and the so-called synovial process—the marginal zone of later periods of development—is seen to be derived from part of this layer.

The existence of vessels in the synovial process is not, in my opinion, any argument for its origin from the inner layer of the capsule, for, as Toynbee's researches shew, these parts are at the time of the fissuring to form the joint all devoid of vessels, which only appear later, and may as readily be conceived to develope in one part as in another. But however that may be, the fact that the epithelioid layer of cells may be in existence, and even disappear long before the appearance of vessels, and without any development of vessels occurring in it (*e.g.* on the concave surfaces), shews that the vessels are of no importance in deciding the question at issue. That they are continuous with those of the synovial membrane, and play an important part in the secreting and absorbing functions of the parts supplied by them, is however undoubted: and, as already remarked, at the margins of the articular surfaces the cells are of an irregular form, and, although derived from cartilage-cells, are similar in appearance to those on the inner surface of the capsule. The function also of the tissue here is doubtless similar to that, and for this reason possesses vessels. So far, then, it may not altogether incorrectly be considered an offshoot of the synovial membrane. It is not, however, merely a membranous layer of cells spread like a carpet over the marginal surface of the cartilage, but is, on the contrary, to be considered an integral part of the latter, for, as already shewn, the processes of the ramified cells actually dip down into the matrix of the cartilage: this may be especially well seen near the inner margin of the "synovial processes," where its cells are disposed in a single layer only. Nearer the synovial membrane the ramified cells are disposed in more than one layer, being placed one over the other, somewhat as in the cornea (fig. 4). The dipping down of the processes is represented in fig. 3: they are often 20 or 30 times as long as the diameter of the cell-body, and pass obliquely inwards into the intercellular substance of the cartilage. The cells, therefore, of the "synovial process" would seem to serve as a medium for the conveyance of fluid, not only between the

blood-vessels and the articular cavities, but also, to some extent, between the vessels and the subjacent cartilage itself.

The investigations which form the basis of this article have been mainly conducted in the Physiological Laboratory of University College, and I would take this opportunity of expressing my thanks to Professor Sanderson for the abundant materials placed at my disposal, as also to Professor Sharpey and Mr Schäfer, to the latter of whom I am indebted for the drawings from which the figures in the Plate have been executed.

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DESCRIPTION OF PLATE IX.

FIGURE 1. Surface of peripheral part of glenoid cavity of 5 inch sheep's embryo (silver preparation).

a. Epithelioid arrangement of superficial cartilage-cells.
b. Transitions from these to
c, *c′*. Saft-canälchen of synovial membrane. Opposite *c* the *Circulus articuli vasculosus* *c′* is taken from the part of the membrane lining the capsule.

FIGURE 2. From the patella of 4 inch sheep's embryo, shewing transitions of cartilage-cells into connective-tissue corpuscles of synovial membrane (silver preparation).

d. Cartilage-cell-spaces.
e. The same acquiring processes.
f. Elongated and irregular spaces of the so-called synovial process.

FIGURES 3 and 4 are from preparations of the marginal zone of the tarso-metatarsal joint of the ox. In figure 3 (nitrate of silver preparation) the cell-spaces, in figure 4 (chloride of gold preparation) the cells themselves, are shewn.

THE ACIDITY OF GASTRIC JUICE. By JAS. REOCH, M.A., M.B. (Edin.), *Demonstrator of Practical Physiology, College of Medicine, Newcastle-upon-Tyne.*

SOME time ago I began a series of experiments on the Sulphocyanide of Potassium, with a view of determining its use in the saliva. Commencing, then, with its reactions on the various Pharmacopœial preparations of iron, I found that while the ordinary ferric salts, such as the Perchloride and Nitrate, gave the blood-red coloration of sulphocyanide of iron readily, the compound iron salts with organic acids did not do so at all; but when a drop of dilute hydrochloric acid was added, the reaction came out at once. There are three salts which in solution exhibit this peculiarity—Ferrum Tartaratum, Ferri et Ammoniæ Citras, and Ferri et Quiniæ Citras; but as the two former are red themselves, while the latter is golden yellow, I did not consider the reaction so delicate or so free from suspicion of error in the former as in the latter, and therefore confined my experiments chiefly to the Ferr. et Quin. Citr., which for shortness I shall call Ferr. Citr. Now, when a solution of this salt, of the strength of 1 gr. or 2 grs. to the ounce, is mixed with a solution of the Sulphocyanide of Pot. of similar strength, no reaction takes place, but the addition of a single drop of dilute HCl develops the red colour of sulphocyanide of iron at once; and the delicacy of this reaction is so great that, conversely, when it is required to know if free HCl exists in a liquid, the addition of this mixed solution will detect it when it amounts to only $\frac{1}{5000}$th part of the total liquid, and where the acid is only $\frac{1}{500}$th part the red colour is so dark as to appear almost black by reflected light; as therefore the amount of HCl in the gastric juice is ordinarily estimated at $\frac{1}{500}$th part, or ·2 per cent., it is evident that this method might give valuable results, because by it an approximate analysis of the gastric juice, in regard to its free HCl, may be made in less than a minute, whereas formerly, by estimating the amount of acid and base separately, and then calculating the excess of acid, the best part of a day

was spent over one analysis, and, after all, the results were not so satisfactory.

But it must first be inquired whether any free acids other than HCl produce this reaction, and also if any substances can destroy or modify it. I found, then, that dilute Nitric, Sulphuric and Tartaric acids produced the reaction as well as HCl; that Carbonic, Lactic, Acetic, Citric, Benzoic and Uric acids had no effect on it, while Phosphoric and Oxalic acids not only did not produce it, but destroyed it when produced. As for the alkalies they, of course, destroy the reaction; but as it is impossible that they should exist in a solution containing free HCl, they may be dismissed at once. Now, the fact that Tartaric Acid produces this reaction, while Ferrum Tartaratum does not do so, seems to shew that what happens is, that the free tartaric acid unites with the potassium, and liberates the sulphocyanogen which in its nascent state attacks the ferric citrate or potassic tartrate, and forms the sulphocyanide of iron; this appears also proved by the conduct of HCl, which, when added to a solution of Ferr. Citr., does not change the colour at all, as it should do if it formed perchloride of iron; and moreover it makes no difference to the reaction whether the Sulphocyanide of Pot. be added first or last; and yet it seems scarcely probable that, when added before the HCl, the latter should form perchloride of iron, and then change the iron for potassium, to enable the sulphocyanide of iron to be formed. I believe it, therefore, to be most probable that this reaction is caused by the liberation of nascent sulphocyanogen. In regard to phosphoric and oxalic acids, which I have described as destroying this reaction, I think the word destroy is hardly applicable, but that the reaction is really produced, as I have satisfied myself by careful experiment, and then dissolved, probably by the neutral phosphate and oxalate of Potassium formed from the Sulph., for, on adding strong HCl to the solution effected by oxalic acid, the colour is restored to a great extent.

While, therefore, this method fails in distinguishing free HCl from free Nitric, Sulphuric and Tartaric Acids, and while phosphoric and oxalic acids in a free state negative the reaction, yet, as these acids are easily tested for separately, and eliminated from an inquiry, the value of this method is scarcely

diminished, more particularly as it holds in the very point in which it is most required, the distinction between free HCl and free lactic acid; for while it is not yet absolutely settled to which acid the gastric juice of man owes its acidity, it is generally believed that in man it is due to free HCl, and in the dog to free lactic acid. Now, during my first experiments on this subject, I failed to find HCl, while I took especial care to prove not only the absence of free HCl, but, by adding an equal quantity of a solution of HCl of the strength ·2 per cent., and obtaining the reaction, I proved that there was nothing in the fluid to prevent the reaction coming out, and that therefore, if free HCl had been present in anything like the quantity ·2 per cent., it could not fail to be detected. These experiments were, however, defective in that they were made on mice, cats, and vomited matter from the human subject; but on establishing a gastric fistula in a dog I obtained, by many scores of experiments, distinct evidences of free HCl. Before, however, describing these later researches, I shall refer to some results which I obtained from the former class of experiments. It appeared to me at that time that a result so much at variance with common opinions must be supported by irrefragable evidence, and therefore I undertook a series of experiments to ascertain if any foreign substance, such as albumen, &c., might interfere with the reaction in any way. It was not probable that such was the case, for I have already mentioned that the reaction came out at once on adding free HCl to the original liquid; but still experiments on that point required to be multiplied. Now, on adding my mixed solution to urine in a test-tube, I observed that no reaction took place, while, if two or three drops of dilute HCl had been previously added, the reaction at once developed itself. The conclusion appeared, therefore, that there is no free HCl in urine, and that if there was, it would shew itself; but on performing the experiment with the greatest care, I found that the reaction did not come out with urine to the same extent as with distilled water if the same quantity of HCl had been added to each. If, for example, six test-tubes be arranged with 2 cc. urine in each, and six test-tubes with 2 cc. distilled water, and if 1, 2, 3, 4, 5, and 6 drops of dilute HCl be dropped into each series, it will be found that in the first test-tube of the

urine series there will be no reaction developed on adding the mixed solution, while the 2nd, 3rd, 4th, 5th and 6th will correspond in depth of reaction more or less with the 1st, 2nd, 3rd, 4th and 5th of the water series, or perhaps the first two of the urine series will give no reaction, and the others correspond with the first four of the water series; obviously, therefore, there is nothing in urine to destroy the reaction, but there is something which delays it and slightly dissolves it; and this something varies within certain limits. The only explanation of this peculiarity seems to me to be that there is something in the urine which absorbs or combines with HCl, and that thus when free HCl is added to urine, though the already acid urine is rendered more acid, yet there is no free HCl till a certain point is reached; now every one knows that when HCl is added to urine it precipitates uric acid by assuming to itself the sodium which the latter is united with; and it is evident, therefore, that when free HCl is added to urine there is formed chloride of sodium and uric acid, and to that extent, therefore, free HCl, when added to urine, will disappear; but it is evident that, the quantity of uric acid in the urine being only 8 grs. per diem, this is altogether insufficient to account for the disappearance of 1 or 2 drops of dilute HCl in 2 cc. urine. I therefore examined carefully the relations of the alkaline and acid phosphates of soda to the sulphocyanide reaction, with the following results:—When a solution of the ordinary or neutral, or rather alkaline, phosphate of soda $Na_2\,H\,PO_4$ is added to a mixed solution of the Ferr. Citr. and Sulph. of Pot., no reaction of sulphocyanide of iron takes place when HCl is dropped into the mixed solution until a quantity of HCl is added, exactly sufficient to convert the alkaline into the acid phosphate, and then it comes out at once, allowing only for a slight solubility of the sulph. in the acid phosphate.

I shall not trouble the reader with an account of the experiments by which I proved this important conclusion, that $Na_2\,H\,PO_4 + HCl$ does not remain so, but becomes at once arranged as $Na\,H_2\,PO_4 + Na\,Cl$, because there is a fact known to every physiologist, which, had it been duly considered, might have proved this long ago. Neubauer and Vogel in their work on the urine say that when uric acid is heated with alkaline phos-

phate it assumes an atom of sodium, becomes urate of soda, and converts the alkaline into the acid phosphate: now every one knows that when HCl is added to urine it appropriates an atom of sodium from the urate of soda, forms chloride of sodium, and precipitates the uric acid. If then such a feeble acid as uric acid can take sodium from the alkaline phosphate and convert it into acid phosphate, is it not probable (and to this I shall afterwards refer) that other acids can do the same, and is it not demonstrably certain that HCl will do so when it can make uric acid itself give up the sodium which it has taken from the phosphate? The dilution of the HCl makes no difference, for though it is added to urine in the concentrated form when uric acid is wanted, that is only because the urine itself dilutes it so much that a teaspoonful of strong HCl in twenty ounces of urine is not any stronger a solution of HCl than the gastric juice at ·2 per cent. Moreover, every one knows that you never get as much uric acid as you add of HCl, simply because, as I maintain, a large quantity of the HCl is required to convert the alkaline phosphate which has not already been converted into acid phosphate. The same reasoning applies to phosphate of lime. The ordinary phosphate of calcium is $Ca_3 P_2 O_8$, and this is usually said to be insoluble in water, but soluble in dilute acids; but, according to my experiments, it is not soluble in HCl except in so far as the latter is added in sufficient quantity to convert it into acid phosphate, which is then dissolved, and thus when HCl is added to $Ca_3 P_2 O_8$ the reaction is $Ca_3 P_2 O_8 + 4HCl = 2Ca Cl_2 + Ca H_4 P_2 O_8$, therefore the HCl disappears until this point is reached, and accordingly I found that it gave no sulphocyanide reaction till this point, and beyond that it gave it in an increasing ratio with the amount of HCl added, excepting only for a very slight solubility of the colouring matter in the acid phosphate. When these facts are fully considered it is evident that Schmidt's results will not hold in a quantitative analysis of gastric juice; for unless you allow that any free HCl present in that juice converts the alkaline phosphates present into the acid phosphates, you will infallibly make the quantity of free HCl much too large, and in Schmidt's analyses the relation of the phosphates to the free HCl is evident, while no relation of the latter to the chlorides is apparent.

	Human G. J.	Sheep's G. J.	Dog's G. J.
HCl	·20	1·55	2·70
Chlorides	2·07	5·98	5·87
Phosphates	·12	2·09	2·73

To return, however, to my first experiments: I examined more than a dozen mice, and found that the reaction of the stomach and its contents was generally acid, and even in some cases post-mortem digestion of the stomach itself was almost complete, and yet in every case I failed to find HCl, though on adding a drop of ·2 per cent. solution to the contents of the stomach the reaction came out. It appears to me therefore a very doubtful matter whether these animals have free HCl in their gastric juice, and probably they have some acid analogous to that which gives them their peculiar smell. In regard to cats also I failed to find free HCl. In one cat I made a gastric fistula; the animal lived for ten days, and I examined its gastric juice several times without detecting HCl; as however it did not live long enough to leave its milk-diet perhaps that might have accounted for the failure. I examined also several specimens of vomited matter from the human subject; after filtration the fluid was generally acid, though in one case neutral, and yet there was no evidence of free HCl; these however might be regarded as abnormal specimens, and I therefore made a gastric fistula in a dog[1]. I was unable to keep a tube in the opening, as the dog injured himself by constantly trying to get it out, and moreover it keeps the part in much better condition to allow the dog to lick the sore. If the opening be not too large the muscular movements of the stomach and abdomen will not allow the food to escape, while a director can be passed and the

[1] My first dog died in three days, and I am persuaded his death resulted from the operation being performed in the way generally recommended, by giving the animal a full meal to distend his stomach prior to the operation, and thus give facility in seizing it. I did so, and found when I opened the abdomen that his stomach was highly vascular. I attached it to the wall of the abdomen by silver sutures, and then opened it, and on his death three days afterwards I found the coats of the stomach highly inflamed, and almost gangrenous. It appeared to me therefore that the danger of exposing the peritoneum too much while searching for the stomach when empty, was imaginary compared with the danger of opening the latter when the coats were highly vascular, and in the full performance of the digestive process. Accordingly, in my second operation, I did not feed the animal for some hours previously, and the result was in every way satisfactory.

fluid will flow along the curve in a much better way than by tickling the inside of the stomach with a feather; for by using the director you can apply it to any part of the stomach at once, and thus compare the results. I kept the dog for a week on milk and bread, and then gradually returned him to a mixed diet. The result of my experiments was that for the first fortnight no HCl was procurable from the juice. I did not examine the juice for a week after the operation to allow the animal time to recover, but during the next week I examined his juice frequently, and failed to find HCl; it then began to appear, and continued more or less during the whole period of my examination.

I found, however, that Schmidt's estimate was much too high for the free acid in the dog. I never found it to rise much above ·2 per cent. under any circumstances, and I therefore conclude that his analyses which gave 2 per cent. did not take sufficient account of the acidity of the phosphates. It appears, however, that free HCl is a nearly constant product in the gastric juice of the dog, varying, however, with the state of the animal; after long fasting it appears in greater quantity than after a limited fast, and its quantity varies also with the kind of food supplied. The question therefore arises, to what is the free HCl due? There can be only three sources of it, from the decomposition of chloride of sodium, chloride of potassium, or chloride of calcium. Many chemists say that it is caused by lactic acid decomposing calcic chloride, but that this is not so is evident, for calcic chloride exists neither in the food nor in the blood, which is a result of the food: to say therefore that HCl arises from the decomposition of calcic chloride just removes the difficulty one step, for what causes the decomposition of sodic or potassic chloride to give rise to calcic chloride in the stomach? Moreover, according to my views the calcic chloride in the gastric juice is easily explained, for it is a result of the free HCl and the phosphate of lime forming acid phosphate and calcic chloride, and, finally, to upset this theory entirely, it is only necessary to deny that lactic acid can take calcium from calcic chloride, much less form HCl from sodic or potassic chloride. I have in my possession a bottle of so-called lactic acid, which undoubtedly decomposes calcic chloride,

but I have satisfied myself that this is from the presence of oxalic acid as an impurity, probably from higher oxidation of lactic acid by defect in the process of preparation, and that this is not a reaction of lactic acid itself. No doubt, this very fact that lactic acid may be oxidized to oxalic acid, may account for the presence of the latter in the urine as a frequent constituent, but that this is the ordinary method of production of HCl is wholly improbable; and I may add, from numerous examinations of the gastric juice microscopically, that I never detected the presence of oxalate of lime. And though oxalic acid were proved present, yet it cannot decompose sodic or potassic chloride, to which I believe the presence of free HCl in the gastric juice is due. Tartaric acid as an agent in the production of HCl, by precipitating the potassium as tartrate of potash, is equally objectionable, for there is no evidence of the presence of tartaric acid in the gastric juice. There is another theory of the production of free HCl which seems to be more popular than the lactic acid theory I have referred to, and that attributes the decomposition to electrolysis; but this theory appears to me even more unsatisfactory than the other, for where is the source of the electricity? Who has proved its existence? and if it does exist, why does it decompose chloride of sodium in particular? why not decompose every other salt? The theory in fact is little better than attributing the origin of HCl to a vital principle. I have made numerous experiments on the effect of the introduction of sodic chloride into the stomach, and uniformly found that it greatly increased the flow of gastric juice, but did not increase the proportion of free HCl above ·2 per cent. In fact it stimulated but did not alter the secretion. It is evident, therefore, that the sodic chloride introduced into the stomach is not decomposed in it by electrolysis, or at least this conclusion is nearly certain, for though I introduced the salt not into the blood, but into the cavity of the stomach, yet it must have passed by diffusion into the blood supplying the glands of the latter; and as I examined the fluid coming from the same part of the stomach, the quantity of HCl should have been increased. The only objection to these experiments is that while adding the electrolyte, I did not add additional electromotive force to decompose it, but still, that being im-

possible, I yet think it forms a strong argument against the theory of electrolysis.

It appears to me that the true theory will best be found by recurring to elementary chemical facts. Albumen contains sulphur, and this is without doubt oxidized in the blood into sulphuric acid, for the nitrogen, &c. of albumen being removed as urea and uric acid, the sulphur is acknowledged by all to be removed by oxidation, conversion into sulphates, and excretion with the urine. Now sulphuric acid is almost the only acid which is able to decompose sodic chloride; and it appears to me therefore highly probable that the oxidation of the sulphur of the albumen takes place in the walls of the stomach during the production of its peculiar secretion, and that this in its nascent state seizes on the sodium of sodic chloride, and allows the free HCl to be eliminated in the gastric juice. It is no argument against this theory that sulphates are not found in the juice, for if the decomposition took place in the walls of the stomach, there is no more reason why sulphates should appear in that secretion than why sodic phosphate, which exists so largely in the blood, should appear in it also. This theory then fully accounts for the free HCl in the gastric juice, and the latter fully accounts for the presence of calcic chloride, as I have already explained[1]. In regard to the phosphate of lime so constantly present in the gastric juice, I do not think that it is a secretory product of the gastric glands themselves, unless in the upper part where they are lined by columnar epithelium, but rather that it is a product of those other glands which secrete the mucus of the stomach. Phosphate of lime is a very constant

[1] As a possible objection to my theory that there is not enough H_2SO_4 in the urine to account for all the HCl in the gastric juice, I may reply, that allowing with Parkes the H_2SO_4 in urine to be 31·11 grs., and only one-third of this to be derived from the food, then since 3—5 grs. of oxidized sulphur are excreted by the urine independently of sulphuric acid, it is evident that even supposing the sulphates produced in the stomach not to be again decomposed, there will be S oxidized equal to about 32 grs. of H_2SO_4, and this would be equal to 12 grs. of HCl, or to 14 ounces of ·2 per cent. solution of HCl. Now though the gastric juice secreted is 10 or 20 times this amount, yet it must be remembered that it is only after long fasting that you can get even ·2 per cent. solution of HCl; ordinarily, as I have remarked, in analyzing vomited matters the acid chyme shews no trace of HCl, which is only secreted for the purpose of digesting flesh-meat, or something similar, and therefore it is quite a mistake to estimate the gastric juice as if each drop of it contained ·2 per cent. HCl. Even in the dog this is rarely the case, for it is not found while giving the animal milk, and little or none when giving it bread.

constituent of mucus, and I think therefore that this is its origin in the gastric juice, though no doubt it is largely increased from the peculiarly irritating nature of the gastric juice which contains so much free acid. But is not lactic acid present in the gastric juice of the dog? I believe it is. Bernard has found it to be so, and I have found in evaporating the filtered juice crystals analogous to those which Scherer describes as those of lactate of lime; but the very fact that I obtained these without adding any calcic carbonate to the solution, shews that lactic acid possesses the property in regard to phosphate of lime which uric acid is known to have with regard to phosphate of soda. It forms lactate of lime and acid phosphate; and this appears to me to be the true function of lactic acid in the gastric juice, and also one reason why starchy matter such as potatoes is so agreeable a mouthful along with flesh-meat, for the starch is converted by the saliva into glucose, and that by the secretion or ferment of the stomach is converted into lactic acid, and thus the alkaline phosphates are converted into acid phosphates without using up HCl, which is so valuable in digestion, and the production of which must task the stomach so highly. But into this subject I will not enter, as experiment is here mixed with theory, and will therefore pass on to remark that numerous experiments which I made shew that the cardiac and not the pyloric end of the stomach is that in which the greatest secretion of HCl goes on; in the latter part, indeed, I scarcely found any HCl at all, but that might possibly be due to the fact that the fistula was near the pylorus, and therefore its presence and some slight inflammatory reaction might have vitiated results. I will add further, that the introduction of sapid substances stimulated the secretion of the juice much more than mere mechanical irritation, and that in such cases the amount of HCl was almost always kept up to nearly ·2 per cent. along with the increased amount of juice, shewing evidently that true stimulation of the stomach is not effected by tickling its inside, but by the substance entering the walls of the stomach by diffusion and stimulating its glands, or perhaps the ultimate filaments of the vagus which control their secretion.

The relation of these facts to the important question of why the stomach does not digest itself is evident. It is not simply

from the alkalinity of the blood, but from the fact that doubtless the nervous system possesses a regulating power over the production of HCl, and that even if the latter were formed in greater excess, its force would be rapidly utilized by union with the alkali of the alkaline phosphate. What chance could HCl have to dissolve a stomach in whose walls alkaline phosphate of soda was constantly circulating? If it attempted to do so it would form simply sodic chloride and acid phosphate, and these would immediately be swept on by the current of blood and excreted by the urine. Under ordinary circumstances, indeed, there is no necessity for this, as the acid phosphate of lime intervening between the gastric juice and the proper coats of the stomach will form a sufficient protection; but should this balance be destroyed there would still not be digestion of the stomach itself, for the reason I have indicated.

NOTE. In the foregoing paper I have constantly spoken of the free HCl in the gastric juice as ·2 per cent. Properly speaking, I should have said ·02 per cent., as that is the proportion found by Schmidt, but though the ordinary books nominally give ·02 per cent., they yet speak of ·2 per cent. in relating experiments on artificial gastric juice, and admit that no less proportion than ·2 or ·1 per cent. will digest fibrin. I therefore assumed that Schmidt's results were wrongly referred to 1000 parts instead of 100, as I had no access to his original paper. If however it be correct to refer them to 1000, then it seems clear that the theory of the origin of the free HCl in the gastric juice which I have proposed by the production of H_2SO_4 from oxidized sulphur will fully account for the free HCl of 7 pints of gastric juice at ·02 per cent.

ADDITIONAL OBSERVATIONS ON THE ANATOMY OF THE GREENLAND SHARK (LÆMARGUS BOREALIS). By PROFESSOR TURNER.

IN June, 1873, I published in this *Journal* some observations on the visceral anatomy of the Greenland Shark, based upon a dissection of two well-grown females, captured in the preceding February off the Bell Rock. Early in March of the present year a young male was hooked on a deep-sea-line off the Isle of May at the mouth of the Firth of Forth. I purchased the specimen from Mr Anderson the fishmonger, and am now able to render my description of the visceral anatomy of this shark more complete, not only by giving an account of the male sexual organs, but in some other particulars.

As the published figures of this shark by Scoresby, Yarrell and Couch, are, in my opinion, unsatisfactory, I requested Mr C. Berjeau, whose skill as a draughtsman is so well known to anatomists, to prepare a drawing of the fish, which he has reproduced on wood to illustrate this paper. The figure is on the scale of one inch to the foot, and has been drawn under my supervision. The colour of the skin was, as in the former specimens, bluish-gray, but the sides were not, as in those fish, barred with faint stripes.

Spiracle placed behind and above the eye, mouth on the ventral surface immediately below the eye. The base of the 1st dorsal fin was a short distance in front of a point midway between the tip of the snout and the tip of the tail. The centre of the base of the 1st dorsal fin was about the mid-distance between the anterior borders of the pectoral and ventral fins. The 2nd dorsal fin was immediately posterior to a line drawn vertically from the cloaca. The 2nd dorsal fin was not only smaller but more pointed at its upper and posterior angle than the 1st dorsal.

In my former specimens, owing to the liver having broken away by its great weight, I was not able to determine if a

gall-bladder existed. In this specimen, which, on account of its smaller size, was much more manageable to dissect, I found a gall-bladder, about twice as large as the corresponding organ in man. It was lodged in a fossa in the liver, and its surface of attachment was overlapped by liver substance. Though it projected from the liver in a well-marked manner, it was not pyriform in shape; its sides were flattened, and it had no neck. Its duct lay for some distance in contact with that part of its wall which was next the liver-substance, and opened into the bladder by a wide orifice. Lying in the fold of peritoneum, which enclosed the bile-duct on its way to the duodenum, was an oval body about the size of a small almond. It had a reddish colour, and possessed the general appearance of a lymphatic gland.

The dimensions were as follows:

	ft.	in.
From tip of snout to tip of axis of tail	6	1
... ... to back of base of 1st dorsal fin	2	11½
... ... to back of base of 2nd dorsal fin	4	7
... ... to anterior border of root of pectoral fin	1	7
... ... to anterior border of root of ventral fin	3	10
... ... to spiracle		8½
... ... to eye		5¼
... ... to mouth		6
... ... to nostril		2½
From cloaca to tip of axis of tail	1	9½
Between upper and lower tips of tail	1	3
Antero-post. diameter of base of 1st dorsal fin		4½
Height of		3¼
Antero-post. diameter of base of 2nd dorsal fin		4
Height of		2½
Antero-post. diameter of pectoral fin		8
... ... of ventral fin		6½

An injecting pipe was introduced into the conus arteriosus and some coloured gelatine was thrown into the branchial vessels. From the sides of the aorta three pairs of branchial

Profile view of a young male Greenland Shark (*Læmargus borealis*), scale 1 inch to a foot. A specimen of the *Lerneopoda elongata* is attached to the right cornea. Engraved by Mr W. Ballingall.

arteries arose, and at its anterior end, immediately behind the basi-hyal, the aorta ended by bifurcating into a terminal pair, each of which, after a course of about an inch, again bifurcated, so that five pairs of branchial arteries resulted. The first or most anterior pair extended outwards along the line of origin of the 1st gill from the hyoid cartilage; the 2nd, 3rd, 4th and 5th pairs ran outwards to their respective gill-arches; each artery lay on the distal side (*i.e.* on the side furthest removed from the heart) of the cartilaginous styles which supported the more posterior of the two gill-laminæ attached to a given branchial arch. A well-defined layer of striped muscle intervened between the trunk of the branchial artery and the more anterior of the two gill-laminæ attached to a branchial arch. The 1st branchial vein at the dorsal angle of the gill-cleft bifurcated, one branch ran forward in a groove on the under surface of the palate, entered a foramen, and assisted in the supply of blood to the head, the other branch joined the more anterior of the two veins proceeding from the gill-lamina lying behind the 1st branchial cleft, and formed the first or most anterior of the roots of the dorsal aorta. The 2nd, 3rd and 4th roots of the aorta were formed in a similar manner by the junction at the dorsal angle of the gill-cleft of a vein from the gill-lamina in front of the cleft with a vein from the lamina behind. In addition to the transverse branches which arose from the dorsal aorta in this region, it gave off two branches at its anterior end, which, diverging from each other, entered foramina in the occipital cartilage.

The testicles were two narrow elongated glands situated at the anterior part of the abdominal cavity. Each gland was connected by a mes-orchium to the ventral surface of the corresponding kidney; the two testicles were separated from each other by the meso-gastric fold of the peritoneum. Each testicle was between 10 and 11 inches in length, $\frac{3}{8}$ths broad, and $\frac{1}{8}$th inch in thickness. It terminated in a narrow pointed extremity at each end. The surface of the testicle was quite smooth, but a narrow band extended along that border which was opposite the line of attachment of the mes-orchium; sections through this band were made, but no trace of a tube was seen within it. From the immature growth of the fish it is probable that the

testicles were not sexually ripe. I carefully examined both the surface of each gland and its mes-orchial fold for an epididymis and a vas deferens. No trace of an excretory duct could be seen. The mes-orchium was so translucent that if a duct had been present I could not but have seen one; nothing indeed was visible between its two layers but a few thin-walled vessels and a little fat. When the mes-orchium was dissected off the kidney a large, long blood-vessel was opened into, the canal of which was subdivided by slender bands.

In the next place I carefully examined the cloaca with the view of determining the number and relation of its openings. The rectum opened by a large orifice into its anterior part. At the bottom of a shallow fossa, immediately behind this orifice, was a small papilla, at the summit of which a minute opening was seen. Into this opening I introduced the fine nozzle of an injecting syringe and forced a coloured fluid into it. This fluid ran freely into two long ducts, one situated on the ventral surface of each kidney, and from the ducts into the kidney substance, but no injection passed into the testicles. The conclusion I came to therefore was, that the minute orifice at the summit of the papilla was common to the two ureters. In this respect the male differs from the female fish previously described, in which each ureter opened independently into the back wall of the cloaca. Where the cloaca approached the surface of the integument two transversely elongated abdominal pores were situated, which freely communicated with the general peritoneal cavity. From this dissection, therefore, it would appear that in the male, as in the female *Læmargus borealis*, the genital ducts are not developed, so that the products of both the male and female genital glands are shed directly into the general peritoneal cavity. As I discussed the importance of this arrangement in connection with the classification of this fish in my previous paper, I need not further refer to it on this occasion.

A cleft $1\frac{1}{2}$ inch in length was situated at the posterior border of the ventral fin. It separated the innermost style-shaped appendage of the basal cartilage of the fin from the other appendages of that structure. The innermost appendage, which was only $1\frac{3}{8}$ths inch long, formed with its tegumentary covering a

short rudimentary clasper. A longitudinal groove was situated on the dorsal surface of the clasper, which passed into a deep cleft lined by a prolongation of soft, smooth integument. No gland was found at the root of the clasper.

As an illustration of the omnivorous character of this shark, I may state that its stomach contained a large haddock, the remains of several herring, a portion of a cuttle-fish, the shell of a dog-whelk containing a hermit crab, a partially digested biscuit, and a boy's playing marble.

ON THE USE OF THE LIGAMENTUM TERES OF THE HIP-JOINT. By W. S. Savory, F.R.S., *Surgeon and Lecturer on Surgery, St Bartholomew's Hospital, late Professor of Comp. Anat. & Physiol., R.C.S.E.*

What is the function of the Ligamentum Teres? Many authors who describe this structure are silent on the question, while of those who answer it the general conclusion is that it has, for its chief function, to limit adduction of the thigh, or when the thigh is fixed, to limit lateral movement of the pelvis on the femur—to prevent the pelvis from rolling toward the opposite side. Authors, of course, are not fully agreed in explanation of its use, and other less prominent functions are by many assigned to it, such as to limit rotation of the thigh; but the conclusion substantially arrived at is the one given above. I cannot, and need not, here quote from the several authors who are entitled to speak with authority on the subject, but neither in them nor elsewhere can I find any allusion to what appears to me to be the prime purpose of this ligament[1]. Its strength is very great; its attachments are remarkable; its situation is peculiar. I submit the following explanation of its use.

When the person is erect the ligament is vertical and tight. This statement, although generally accepted, has been challenged. I am satisfied of its accuracy. By removing the bottom of the acetabulum from the pelvis with a trephine the state of

[1] Since this paper has been in type I have learnt that Prof. Partridge in his lectures on anatomy at King's College was accustomed to compare the Ligamentum Teres, in its function, to the leathern straps by which the body of a carriage is suspended on C springs; and my attention has been called to the following passages published by Prof. Turner in 1857 (*Human Anatomy and Physiology*, Edinburgh): "In the interior of the joint (hip) is a strong band of fibres called the inter-articular or suspensory ligament. When a person is standing erect or with the body slightly bent, a portion of the weight of the trunk is borne directly by the heads of both thigh-bones, or of one thigh-bone, according as he stands upon one or both legs, owing to the direct pressure of the acetabulum upon the heads of those bones. Now as the end of this ligament, which is connected to the lower margin of the acetabulum, is much lower than the end connected to the thigh-bone, it of necessity suspends that portion of the weight of the body which is thrown upon it. The effect of this is to distribute over the head of the thigh-bone that weight which, supposing the suspensory ligament had not been present, would have been sustained by that portion merely which is in direct contact with the upper part of the acetabulum." (p. 42.)

the ligament may be demonstrated. But I think the discrepancy of observation is due to the fact that the degree of tension of the ligament is dependent on the line of direction of the femur. The ligament is moderately tight when a person stands evenly upon both legs. It is tighter when the femur is slightly flexed as it more usually is. But when resting upon one leg, inasmuch as the pelvis is then raised on that side, which of course affects the ligament in the same way as adduction of the femur would do, then the ligament becomes extremely tense. In other words, it becomes tightest when the hip-joint has to sustain the greatest weight.

When therefore the pelvis is borne down upon the femur, or when the femur is forced upwards—that is, when the pressure would be greatest between the upper part of the acetabulum and the opposite surface of the head of the femur—it is put directly on the stretch. More precisely, its great purpose is to prevent undue pressure between the upper portion of the acetabulum, just within the margin, and the corresponding part of the head of the femur. But for this ligament such undue pressure must inevitably occur. Suppose the Ligamentum Teres absent, and the person standing upright: owing to the obliquity of the acetabulum and the head of the femur—of the axis of the joint—pressure between the two could not be equally, or nearly equally, diffused over their opposing surfaces, but it would be concentrated on a spot in the upper part of the socket through which a line drawn down the body, through the joint into the leg, would pass. When the thigh is straight, when the femur is in a line with the body, as when one stands upright, then is the Ligamentum Teres in the same line too, and consequently any force which drives the femur and pelvis together must tell at once upon the ligament, and be directly checked by it.

Owing, therefore, to the shape and obliquity of the hip-joint, and the weight of the body, the Ligamentum Teres is necessary to prevent concentration of pressure at a particular point above it.

The line through which the weight or force acts between the upper part of the acetabulum and the opposed surface of the head of the femur forms, with the line of weight or force which passes through the Ligamentum Teres, an obtuse angle; and the

resultant of these forces is in a line which passes through the long axis of the head of the femur.

When the person is erect the body partly hangs upon the Ligamentum Teres.

I submit that this is the prime function of the Ligamentum Teres. Other purposes I do not deny, but would maintain that they only occasionally come into play, and are altogether subordinate to this one, which is especially called into action whenever the weight of the body is thrown upon one leg.

Now this view may be tested by the facts of comparative anatomy.

It has often been remarked that the Ligamentum Teres is apparently distributed among animals in a very arbitrary manner.

In most of the mammalia it is present, *e.g.* in ruminants, rodents, and terrestrial carnivora. In many other absent, *e.g.* in the elephant, sloth, seal, walrus, sea-otter, ornithorhynchus, and echidna.

It exists in animals with the utmost diversity of form and habits.

It is sometimes present in one animal, *e.g.* the chimpanzee, and absent in another very closely related to it, *e.g.* the ourang-outang[1].

Now is it possible to discern the conditions under which it is present or absent?

When the cavity of the acetabulum looks downward and the head of the femur upward, in other words, when the direction of the hip-joint is nearly vertical, and the weight of the body falls through the centre of the joint, then the Ligamentum Teres is absent, *e.g.* elephant.

When the acetabulum looks outward and the head of the femur is inclined inward, in other words, when the hip-joint is placed obliquely, so that there would otherwise be undue

[1] There is great difference in the degree to which the Ligamentum Teres is developed in Birds. In some it hardly appears, while in many it is very strong. The great depth of the groove in the head of the femur of the Ostrich shews the size it occasionally attains. In several birds in which I have dissected this ligament I have always found its pelvic attachment to be, not to the border of the acetabulum, but to the lower margin of the large foramen or foramina which exist at the bottom; this, so far as its action is concerned, comes to the same thing.

pressure at a particular part, then the Ligamentum Teres is present, *e.g.* horse.

The exceptions to this occur in those animals in whom, although it is an instrument of progression, the posterior extremity does but little in supporting the weight of the body, *e.g.* seals, and the ourang-outang.

These facts, that while the Ligamentum Teres is found in the chimpanzee and other monkeys, it is almost or entirely wanting in the ourang-outang, at first sight apparently so capricious, are very suggestive. It is easy, I think, to understand why it is generally present in monkeys, inasmuch as in them the hip is placed obliquely, and the posterior extremity can support the trunk. But the hip-joint is oblique also in the ourang-outang. The conformation of the foot, however, is the key to the explanation of its absence here. It is clear that in the ourang-outang the posterior extremity cannot be such an instrument of support to the trunk raised upon it, as in the chimpanzee, and consequently the Ligamentum Teres is not needed to counteract undue pressure at a particular point.

Again, it may be said that when an animal stands, in proportion as the long axis of the head of the femur approaches to a vertical line, so does the Ligamentum Teres become weak, until it disappears. On the contrary, it is strongest where the head of the femur has a direction farthest from the vertical, and has to support the greatest weight.

In conclusion, I should like to call attention, without attempting to lay too much stress on it, to a specimen (2. 43) in the pathological series of the museum of St Bartholomew's Hospital, which is thus described in the catalogue.

"Two hip-joints from the same person. In each joint the Ligamentum Teres is completely wanting. The capsule of each is perfect and exhibited no appearance of disease. In the usual situation of the attachment of the Ligamentum Teres there is a deep depression in the head of the femur, and just above this the cartilage of each femur is slightly absorbed."

It may be observed that the cartilaginous shell on the head of the femur is naturally thickest on the upper and inner aspect.

The above Paper was read at a Meeting of the Cambridge Philosophical Society, in April, 1874.

In discussions which followed Professor Humphry, after expressing his obligation to Mr Savory for affording the opportunity of discussing the subject with him, observed that the suggestions made with reference to the function of the ligament by Mr Savory rested entirely upon the view that the ligament is tight in the erect posture. Professor Humphry was one of those who had challenged this view, of the accuracy of which Mr Savory had expressed himself to be satisfied. He referred to his work *On the Human Skeleton including the Joints*, in which he had stated as the result of careful observation that the ligament is not tense and cannot be rendered tense in the erect posture. He had lately reconsidered the question and re-examined the specimens, or some of them, upon which his statement had been based, as well as other recent specimens made for the purpose, and he was convinced that it was correct. In the first place, the dimple in the head of the femur for the ligament is more or less oblong or pear-shaped, and is directed from above downwards and backwards with such obliquity that the ligament can lie in it, as it must do when it is in a state of tension, only in the semiflexed position of the hip, the thigh being inclined from the vertical to an angle of about 45°. This can be seen in the dry bone, and still better in recent specimens in which the direction of the insertion of the fibres of the ligament are seen to correspond with this view. Secondly, the trephine hole through the bottom of the acetabulum shews clearly that it is at about this angle only, and when the thigh is adducted, that the ligament is really tense. In the erect posture, and by the erect posture he meant when the thigh descends vertically from the pelvis and the capsular ligament, more particularly the anterior part of it, is tight, neither adduction, nor rotation, nor any other movement will throw it into a state of full tension. If this is so, which the several specimens examined by the Professor proved to be the case, then it is quite certain that the body cannot hang upon the Ligamentum Teres when the person is erect, and the inferences based upon such a view fall to the ground.

When resting upon one leg the body is tilted a little over to that side so as to throw the line of gravity more directly over that limb, the opposite side of the pelvis is slightly raised, the movement being equivalent to that of *ab*duction of the limb upon which the weight is borne, and the Ligamentum Teres is not stretched, but is still more relaxed than in the erect posture. Even in the position of 'stand at ease,' when the weight is borne upon one limb and the opposite side of the pelvis is lowered, the other limb being placed upon the ground slightly flexed, the movement now being equivalent to that of *ad*duction of the weight-bearing limb, though the Ligamentum Teres is less relaxed than in the former position, and is also less relaxed than in the erect posture, still it is not tight; and the body is slung, not

upon the Ligamentum Teres, but upon the thick and strongly resisting upper portion of the anterior ligament of the hip. The use of the ligament the Professor believed to be, as he had stated in his work, to assist in bearing weight when the limb is placed upon the ground partially flexed and adducted, when the capsule of the hip is comparatively relaxed, and when, if the body be overweighted, dislocation is most likely to occur. In estimating, however, its value, even in this position, it must not be forgotten that several instances have occurred, some of which are noted in Meckel's *Archiv*, VI. 341, in which the ligament was wanting without its being known that any inconvenience had resulted from its absence. In dislocation, too, it must be severed, and it is highly improbable that it ever unites. It has been found indeed ununited. Still the loss of it does not appear to be much felt.

With regard to other mammals the ligament as stated in the paper is commonly absent when the lower limbs do not bear much weight, and also when they descend vertically from the pelvis. The Professor had, however, pointed out in the *Journal of Anatomy*, Vol. III. p. 312, that it is present in the Bats. In most mammals in which it exists the dimple or furrow or angular depression which it occupies in the head of the femur is oblique, as in Man, indicating its tension in them, as in him, to occur in the semiflexed position of the joint.

There were other points to which the Professor took exception, but the important one was this of the position of the joint in which the tension of the ligament takes place.

Mr Savory, in reply, remarked that he quite agreed with Professor Humphry that, if he were wrong as to the assumption of the tension of the ligament in the erect posture, his view fell to the ground, but he could not agree with the Professor as to what really is the erect posture. The skeletons in museums are commonly articulated wrong, and give too much inclination to the pelvis, and he thought Professor Humphry was in error on this point, and that if the ligament be examined in the strictly erect posture it will be found tight, or more nearly so than the Professor admitted. He added that by applying his view he had generally been able to judge from the direction of the limbs in well-articulated specimens of animals whether the ligament had been present during life or not. Still there were some exceptions, among the most notable of which was the difference between the ostrich and the emeu. In the former it is large, whereas in the emeu it is absent. Yet, though he had visited the latter animal in the Zoological Gardens, and examined its posture and movements with reference to this question, he had been unable to make out why it should thus differ from the ostrich.

FURTHER EXAMPLES OF VARIATIONS IN THE ARRANGEMENT OF THE NERVES OF THE HUMAN BODY. By PROF. TURNER.

SINCE my last paper in this *Journal* on variations in the arrangement of the nerves (November, 1871) some additional examples have come under my observation, and that of some of my pupils.

Fourth cranial nerve.—In an adult male, Mr W. J. Dodds observed in the right orbit a well-marked branch to arise from this nerve close to the superior oblique muscle, which branch ran forward parallel to that muscle as far as the upper border of the orbit, where it branched, the branches entering the orbicularis palpebrarum. This muscle was then dissected under spirit with needles, and the finer subdivisions of the nerve were traced into the fasciculi of the muscle, at the inner part of the upper eyelid. In the opposite orbit the fourth nerve was normal.

In an adult female subject, Mr H. S. Stone dissected in the left orbit a small branch arising from the fourth nerve immediately after it had passed through the sphenoidal fissure. It ran forwards, close to the inner side of the superior oblique, and about three-fourths of an inch from the upper orbital border formed a plexus with the infra-trochlear branch of the nasal: from this plexus branches arose, those most in line with the branch of the fourth passed to the orbicularis palpebrarum external to the trochlea, and immediately beneath the orbital arch, those most in line with the infra-trochlear were traced to the mucous membrane of the upper eye-lid. The nerve on the opposite side was normal. Murray, as quoted by Henle, had also seen one case in which a branch of the fourth communicated with the infra-trochlear. In another adult female, Mr Stone traced in the left orbit a branch from the fourth nerve, which ran forward parallel to the outer side of the superior oblique, and near the upper orbital border broke up into filaments which entered the periosteum of the anterior part of

the roof of the orbit immediately to the outer side of the trochlea.

Cervical Plexus.—In my former paper in this *Journal,* I recorded a case in which the middle of the three supra-clavicular nerves passed through a hole in the left clavicle in its course to the integument covering the pectoralis major. I have since then seen two other cases, one on the left, the other on the right side, the hole in each case being about the middle of the clavicle. This variation is obviously more common than is usually supposed. Henle mentions that Bock, Gruber, Luschka, Clason and Cruveilhier, have all recorded cases. The formation of bone around this nerve as it crosses the clavicle is due to the shaft of the clavicle being developed out of membrane, and in all probability to the extension of the ossific process into the fibrous tissue, which surrounds this descending branch of the cervical plexus.

Brachial Plexus.—Mr Stone has especially examined, during the past winter, the arrangement of the nerve to the subclavius. Commonly it gave a branch to the phrenic; most frequently, a small branch ran almost transversely to join the phrenic before that nerve entered the thorax. In one case, the nerve to the subclavius gave origin to a long branch which entered the cavity of the thorax a little in front of the phrenic and nearer the mesial plane ; it descended in front of the arch of the aorta and root of the left lung, along the side of the pericardium, and joined the phrenic immediately above the diaphragm. In another case, two branches arose from the nerve to the subclavius, both of which entered the thorax, and joined the phrenic before it crossed the arch of the aorta. These cases examined by Mr Stone, conjoined with those previously described by myself and by Mr Cunningham[1], sufficiently establish that accessory roots to the phrenic nerve are by no means uncommon. In another case the nerve to the subclavius gave off a branch which joined the external anterior thoracic, and a second branch which proceeded to the clavicular part of the sterno-mastoid muscle.

[1] This *Journal,* November 1871 and November 1872.

In the left upper arm of a subject, used in illustration of my lectures on the nervous system, I saw the ulnar nerve give origin to a slender branch about one inch below the tendon of the latissimus dorsi, which after a course downwards of about two inches joined a branch of the internal cutaneous nerve, and with it was distributed to the skin of the inner side of the upper arm, immediately above the elbow. In another subject, the internal cutaneous nerve for the back of the left fore-arm arose from the ulnar in the lower part of the axilla: about one inch above the elbow it received a very slender branch of communication from the internal cutaneous nerve. In the right arm of another subject, the digital nerve for the ulnar side of the ring-finger arose about the middle of the fore-arm from the ulnar nerve and passed as a long slender branch in front of the annular ligament to its distribution.

Sacral Plexus. In the left leg of a male subject the musculo-cutaneous nerve gave off in the substance of the peroneus longus a long slender branch which descended in the substance of that muscle to about the lower third of the leg, then pierced the muscle, but continued under the fascia as far as the external malleolus, where it became superficial, and joined the external saphenous nerve.

THE SYNTHESIS OF MOTION, VITAL AND PHYSICAL. By C. B. Radcliffe, M.D.

Introductory Remarks.

More than five and twenty years ago my faith in all that I had been taught to believe about vital motion received a rude shock in this way. I had been watching the phenomena of fatal tetanus in a rabbit poisoned by strychnia: I saw the animal die: I expected to see its muscles relax at this time: I was unable to find the slightest evidence of such relaxation until the end of the fourth day after death, when putrefaction had evidently set in. Over and over again, by feeling them with my fingers, did I satisfy myself that the muscles remained rigid all this while; and I had also additional evidence to the same effect, in the fact that in dying the animal did not fall down from a position in which it had been held up by the spasms during life, and from which it must have fallen, as it fell finally when the muscles were unstrung by putrefaction, if death had been attended by muscular relaxation. Indeed, it was this latter fact which arrested my attention in the first instance, and made me use my fingers as I have said. For until the muscular texture gave way in actual decomposition, it so happened that the animal remained in the position in which it had been kept by the spasm during life, that is, half-standing on its hind legs, and half-leaning against the side of a box, with its forepaws pointing directly upwards, and with its neck and body bent backwards until the head almost rested upon the scut—a position which could not have been preserved for a single moment if the body had not been propped up by the box as well as rigid. And how was this? Could it be that the spasmodic rigidity which existed before death had passed without any interval of relaxation into the cadaveric rigidity which always comes on, sooner or later, after death, and which is only relaxed by the actual decomposition of the muscular tissue? Could it be that spasm had passed directly into *rigor mortis?* All my prejudices were against such a notion, and

yet I could not bring my mind to think otherwise. Indeed, the more I tried to do so, the more I felt constrained to assent: and, in short, almost before I knew what I was about, I came to believe that a radical change was necessary in the doctrine of vital motion—that the interpretation of spasm was to be sought, not on the side of life, but on that of death, even in *rigor mortis*—that spasm and *rigor mortis* were to be regarded, not as signs of vital action in certain vital properties of contractility, but as physical phenomena akin to, if not identical with, the return of an elastic body from a previous state of extension—that muscular contraction in all its forms might be the simple consequence of the operation of the natural attractive force or forces inherent in the physical constitution of the muscular molecules—that life is concerned in antagonizing contraction rather than in causing it—that this antagonizing influence itself might have a physical basis—that, in short, a true doctrine of vital motion involved an actual synthesis of vital and physical motion.

And yet more did this conviction grow in strength on the food supplied by two other facts to which my attention was called not long afterwards.

Of these two facts the first was brought to light in an epileptic patient in whom it had been thought expedient to try and cut short a succession of most violent convulsions by taking blood from the temporal artery. The artery was divided when the fit was at its height, and the blood escaped by jets in the usual way, but not of the usual colour. Instead of being *red*, the blood was *black*: that is to say, instead of being *arterial*, it was *venous*. The state during the convulsion was evidently that of suffocation: and on this account, black, unaërated blood had found its way into the arteries, and was being driven through them at the time. The case was intelligible enough as regards the suffocation, for in this state the simple fact is, that black blood does for a time penetrate into and pass along the arteries; but it was not intelligible as regards convulsion, if convulsion was, as it is assumed to be, a sign of exalted vital action. I could connect such exaltation with increased supply of *red* blood to certain nerve-centres, but not with the utterly contrary state of things involved in the actual

circulation of *black* blood; and, do what I would, I could see no other conclusion open to me than that which had been already forced upon me by the history of the poisoned rabbit, namely this, that the convulsion pointed to a state of things which was the very reverse of vital stimulation, even devitalization—that, in short, the state of muscular contraction was due, not to the *black* blood having acted as a stimulus, but to the simple withdrawal of an *inhibitory* action which had served to keep up the state of muscular relaxation so long as the system was duly supplied with *red* blood.

And so likewise with the second of the two facts to which I have alluded. I had the good fortune to be present on one occasion when Matteucci was experimenting with strychnia upon the electric ray of the Mediterranean, and to hear some of the reasons advanced by this excellent physiologist for supposing that there was an intimate connection between the action of the electric organ and muscular action: and, being very much struck with the fact, that the discharge of the electric organ caused by the poison was always accompanied by spasm, I could not help but wonder whether muscular contraction might not agree with the action of the electric organ in being accompanied by discharge—whether muscular relaxation was not accompanied and produced by charge—whether muscular contraction did not hold the same relation to discharge, the discharge acting, not as a stimulus to a vital property of irritability in nerve or muscle, but simply by allowing the play of the attractive force inherent in the physical constitution of the muscular molecules—a play which had been previously counteracted by the presence of the charge. I could, indeed, bring myself to adopt no other conclusion than this: and thus it was that this experiment upon the Torpedo proved to be the means of adding not a little strength and definitiveness to the conviction at which I had already arrived respecting vital motion.

The question at issue, however, was not to be disposed of so summarily; and, as I can now see plainly enough, I had long to wait before I had evidence upon which my own reason or that of any other person was entitled to rest with any feeling of real satisfaction. Indeed, it is only of late that I have had

such evidence in my possession, and that I could safely venture to challenge attention to it—that I could do what I propose to do so soon as I have prepared the way a little by glancing at certain historical points, which, as it seems to me, will make it somewhat more easy to abandon the view of vital motion which is at present in favour, and to adopt that which I would venture to substitute for it.

Very misty notions about vital motion prevailed in olden times, and if the subject be looked into historically it will be seen that much of this mistiness still clings about the notion at present in favour.

In the beginning, as it would seem, all motion was looked upon as essentially vital.

Thales talked about the world as being animated by a soul and actuated by demons, and looked upon motion as being brought about in one or other of these ways.

Hippocrates believed in the universal presence of a living, intelligent, active principle, to which he gave the name of *nature* (φύσις), and to him, as to many in the present day, it was enough to refer motion to *nature*—to regard it as *natural*. The power of motion, indeed, was one of the faculties (δυνάμεις) with which the principle of nature was endowed.

Plato says little to the point. With him all philosophy merged in theology: to him vital motion, and motion generally, resolved itself into a display of divine power.

Aristotle, the great contemporary of Plato, recognized, not a Divine Being as Plato did, but a *First Moving Cause, a primum mobile,* one in essence, eternal, immaterial, immoveable, and yet the spring of all motion. According to him, this First Moving Cause worked in the living body (ζῶον) through the instrumentality of a principle which was distinctive of this body, and to which he gave the name of soul (ψυχή)—a principle possessing various energies or faculties of its own, distinct from the organs in which it was manifested, and yet requiring these organs for its manifestations. To this soul, when most developed, belonged several faculties (δυνάμεις)—the faculty of receiving nourishment (δύναμις θρεπτική), the faculty of sensation (δ. αἰσθητική), the faculty of motion in place (δ. κινητική), the faculty of impulse or desire (δ. ὀρεκτική), the faculty of intel-

ligence (δ. διανοητική). Vegetables even, by having the lowest of these faculties, the threptic, were supposed to have souls. And—chiefly by chancing to witness the action of the intercostal muscles under the transparent pleura in a living chameleon, which he had cut open—Aristotle was also able to connect the movement of animal bodies with the action of the muscles, and to indicate, not only the difference in function between the muscles and the nerves, but to define more clearly—a discovery hinted at by Praxagoras two hundred years before his time—the distinction between nerves of motion and nerves of sensation.

After this time, for a thousand years and more, when anything was done in this direction it was little beyond a servile copying of what had been seen said by Hippocrates and Aristotle. Even Galen made little progress in the matter of originality; nor yet the schoolmen of the middle ages, with whom for the most part the notions chiefly in the ascendant were those of alchemy and magic and astrology. At the revival of letters, indeed, the only light of importance was that derived from the old Greek fathers in science; and at the end of this epoch no new light had arisen to dissipate the darkness. No new light, for instance, was shed by the doctrine of occult causes which found most favour in these times, for this doctrine was no more than a copy of the doctrines of Hippocrates or Aristotle, that various bodies had various powers (δυνάμεις) by which they were able to act in the various ways *natural* to them. And still less did new light proceed from the notion of Paracelsus and his followers, which in too many instances was associated more or less closely with that of occult causes—the notion, that is to say, that there were elementary spirits, intermediate between material and immaterial beings, with special names, in the four elements of air, water, fire, and earth—sylvans or fairies in the air; nymphs and undines in the water; salamanders in the fire; gnomes, trolls, pigmies, spirits of the mine, little folks, little people, cobolds in the earth;—that the human body had its double or dæmon, called Archæus, whose primary function was to superintend the work of the stomach, and who managed the various functions of the body, that of motion included, through the instrumentality of a legion of underling deputies undignified by any distinctive names.

Indeed it was not until Von Helmont, Stahl and Hoffmann appeared on the scene that the notions handed down from the ancients began to be materially modified, and to take the shapes belonging to modern times.

With Paracelsus, Von Helmont held that the Archæus and its underlings were the agents in all vital manifestations, and he also thought for himself a little, for to him belongs the credit, if credit it be, of being the first to maintain that the living body had powers of a specific character altogether different from those belonging to inanimate nature.

Accepting the doctrine that there was one law for animate and another for inanimate nature, Stahl went further, and maintained that matter is essentially and necessarily passive and inert, and that all its active properties or powers are derived from a specific and immaterial animating principle imparted to it—a principle to which he gave the name of *anima*. The body, he held, as body, has no power to move itself. All vital motion is a spiritual act. The physical powers of matter, which have only free play after death, are in every way opposed to, and counteracted by, the *anima*, of which he further says, as the followers of Hippocrates said of *nature*, that "it does without teaching what it ought to do," and does it "without consideration;" a remark which makes it evident that the *anima* of Stahl is not to be confounded with the conscious personal Archæus of Paracelsus and Von Helmont.

What Stahl explained in this way, Hoffmann, who took the next noticeable step in advance, explained on the hypothesis of *nervous influence* or *nerve-fluid*, whatever that may mean. By this influence or fluid, according to him, the moving fibres have a certain power of action, or tone, which may be increased or diminished. If increased unduly, spasm is the result: if unduly decreased, atony.

Next in order have to be named Glisson, Haller, and *the* Brown, known as the author of the Brunonian system of medicine, men whose speculations form the basis of the doctrine of vital movement now in favour.

Glisson, an eminent professor at Cambridge in his day, was the first to advance the present doctrine of *muscular irritability*. He asserted that there was in muscle a specific vital property,

to which he gave this name, and that contraction was the act of this property.

Haller expanded this idea, and drew for the first time a line of distinction between the special vital property of muscle and the special vital property of nerve. He retained the name of irritability for this property in muscle: he gave the name of sensibility to this property in nerve. Each property was essentially vital, something departing at death, and therefore in no wise akin to any power in inanimate nature. The property was a *life* of which muscular contraction or nervation were *acts*.

Brown, starting from this point, added another idea—that of *stimulation*. Everything acting upon the vital property of irritability or sensibility (to which he gave the common name of *excitability*), according to him, acted as an excitant or stimulus. Action is caused by a process of stirring-up, as it were, the capacity for action being asleep or at rest until it is so stirred-up. The idea would seem to be none other than that all vital movement in its nature is identical with that which is produced by teasing a sleeping man until he wakes up and strikes about him in anger.

And this doctrine of vital motion, which thus took form in the speculations of Glisson, Haller, and Brown, is, with little change, the doctrine at present in favour.

In point of fact, the position taken at present has but little shifted since the days of the schoolmen, when occult qualities of one kind or another were thought to be a sufficient explanation for everything—when, for example, terreity, aqueity, and sulphureity, the occult qualities of the three elements, earth, water, and sulphur, of which, in varying proportions, according to Paracelsus, all bodies were composed, were supposed to account for all that was general in these bodies,—when Petreity was thought to be a sufficient explanation of the peculiarities distinguishing Peter from men with other names,—when the answer of Argan[1] to the question, "quare opium facit dormire," in the mock examination for the diploma of physician, would have been listened to without a smile if it had been given in sober earnest before the examiners of a real faculty of medicine:—

[1] Molière, *La Malade Imaginaire:* 3ième intermède.

Mihi a docto doctore
Demandatur causam et rationem *quare*
Opium facit dormire.
Et ego respondeo
Quia est in eo
Virtus dormitiva
Cujus est natura
Sensus assoupire.

For in referring vital motion to a property of irritability, what more is done than to say, that the moving body moves because it is actuated by an occult quality which is suspiciously akin to terreity, aqueity, or sulphureity, or to Petreity, or to the "virtus dormitiva" of opium in the comedy? "To tell us," as Newton said, "that every species of thing is endowed with an occult specific quality, is to tell us *nothing*." Even to say that the phenomenon is *vital*, is, as Whewell remarks, "very prejudicial to the progress of knowledge by stopping enquiry *by a mere word*." Moreover, the very assumption upon which the doctrine in question is based—that vital motion is altogether distinct from physical motion—is itself not altogether satisfactory. "At the best," as Coleridge says[1], "it can only be regarded as a hasty deduction from the first superficial notions of the objects that surround us, sufficient, perhaps, for the purpose of ordinary discrimination, but far too indeterminate and diffluent to be taken unexamined by the philosophic enquirer. * * * * By a comprisal of the *petitio principii* with the *argumentum in circulo*—in plain English, by an easy logic which begins by begging the question, and then, moving in a circle, comes round to the point where it begins—each of the two divisions has been made to define the other by a mere re-assertion of their assumed contrariety. The Physiologist has luminously explained $y+x$ by informing us that it was a somewhat that is the antithesis of $y-x$, and if we ask what then is $y-x$, the answer is, the antithesis of $y+x$;—a reciprocation that may remind us of the twin sisters in the fable of the Lamiæ, with one eye between them both, which each

[1] *Hints towards the Formation of a more Comprehensive Theory of Life.* By S. T. Coleridge. Ed. by Dr Seth B. Watson. Churchill, 1848.

borrowed from the other as either happened to want it, but with this additional disadvantage, that in the present case it is, after all, but an eye of glass."

But this glance at the history of vital motion is not yet ended. Up to the time of Von Helmont the idea of a well-defined gulph between animate and inanimate nature was not clearly defined: nor yet after this time did this idea gain universal acceptance.

At the time of Paracelsus the facts of chemistry began to occupy a large share of the attention of philosophers, and soon afterwards a school, called the Iatro-chemical school, propounded various physiological doctrines founded upon chemistry. The opposition of acid and alkali, and the workings of ferments of one kind or another, were supposed to supply the solution of many problems in vitality. Then came the hope, kindled naturally by the splendid discoveries of Galileo and Newton in physical science, that the mechanical principles of the macrocosm would supply the key to all requiring interpretation in the microcosm—a hope which called into existence the so-called Iatro-mathematical or mechanical Physiologists. The question was of the cohesion, the attraction, the resistance, the gravity, which operate in inert matter, and of mechanical impulse and elasticity, not of powers of a higher order: it was believed that all the various bodily functions were problems to be solved, as so many hydraulic or hydrostatic problems chiefly, partly by gravitation and the laws of motion, and partly by chemistry, which itself, as far as its theory was concerned, was but a branch of mechanics, working exclusively by imaginary wedges, angles and spheres. The restoration of ancient geometry, aided by the modern invention of algebra, had placed the science of mechanism on the philosophical throne. It was thus, for example, that Borelli dealt with the problem of muscular motion, and after him Bellini.

As far back also as the time of the great Bacon, Gilbert had struck out a new path in the same direction, the following out of which has led to more satisfactory results than any of those arrived at by the Iatro-mathematical School in their own particular lines of enquiry. He had investigated the phenomena of magnetism with great success, and by continual poring

over this subject had come to believe that magnetism supplied the key to vital movement, and to vital and physical problems in general; but his speculations bore little or no fruit, and are chiefly of interest as being the first step in an enquiry which did not begin until two hundred years later, when an event occurred in a house in Bologna which marks the birth of a new epoch in the philosophy of vital motion, and on which it may be well to say a word or two. The house is in the Via Ugo Bassi, gia Strada Felice. The event is commemorated on a marble slab let into the front in these words:—

LUIGI GALVANI
in questa casa
di sua temporaria dimora
al primi di settembre
dell' anno MDCCLXXXVI
scoperse dalla morte rane
LA ELETTRICITA ANIMALE
Fonte di maraviglie
a tutti secoli.

The actual event was this. Experimenting with an ordinary electrical machine at a short distance from a dish on which lay a number of frogs' legs prepared for cooking, and noticing that these legs jumped whenever he drew a spark from the prime conductor of the machine, it occurred to Galvani that they might serve as very delicate electroscopes in some experiments on atmospheric electricity in which he was then engaged. Thereupon, he and his nephew Camillo Galvani, who was with him at the time, each of them taking with him a handful of the contents of the dish, mounted to a belvedere on the top of the house which served the purpose of an electrical observatory, and at once proceeded to put the idea in practice. It was expected that these limbs might jump in obedience to discharges of atmospheric electricity as they had been seen to jump in obedience to discharges of Franklinic electricity; and in order to see whether they would do so or not, they were suspended, by means of small hooks of iron wire bent suitably upon certain iron bars or stays which stretched across the upper part of the arched openings with which three sides of the belve-

dere were pierced. The time was a clear and calm evening, without a gleam of either lightning or aurora; and yet the limbs were found to jump whenever the iron hooks by which they were suspended were pressed upon by the finger, and not unfrequently when they were left untouched. Describing what happened, Galvani says, "Ranas itaque consueto more paratas uncino ferreo earum spinali medulla perforata atque appensa, septembris initio (1786) die vesperascente supra parapetto horizontaliter collocavimus. Uncinus ferream laminam tangebat: en motus in rana spontanei, varii, haud infrequentes. Si digito uncinulum adversus ferream superficiem premeretur, quiescentes excitabantur, et toties ferme quoties hujusmodi pressio adhiberetur[1]." The house, the ricketty wooden flight of steps leading from the principal staircase to the belvedere, unmended, unpainted, almost unswept, the belvedere itself, the iron bars upon which the limbs were suspended, are still there, or were there the other day when I made a pilgrimage to the spot; and even the presence of Galvani himself may be recalled by the help of a portrait which hangs or hung in the open landing facing the locked door at the foot of the stairs leading to the belvedere. In this place, and in this way, was the discovery made which is commemorated on the slab on the front of the house as the well-spring of wonders for all ages "fonte di maraviglie a tutti secoli," and of which a short time before the close of the last century the illustrious author of *Cosmos* wrote "le nom de Galvani ne perira point; les siècles futurs profitèront de sa découverte, et, comme le dit Brandes, ils reconnaitront que la physiologie doit à Galvani et à Harvey ses deux bases principales[2]." At this time, then, and in this place, Galvani saw the contractions he describes, and discovered, or rather divined, in them the existence of animal electricity. How, he asked himself, were these contractions to be accounted for? They could not be due to discharges of atmospheric electricity, for the sky at the time presented no indications of electric disturbance: they could not be due to the discharges which gave rise to them within the house,

[1] *De Viribus Electricitatis in motu musculari Commentarius*, 1791.

[2] *Expériences sur le galvanisme, et en général sur l'irritation des fibres musculaires et nerveuses.* F. A. Humboldt. Traduit par J. F. N. Jadelot. 8vo. Paris, 1799, p. 361.

for the electric machine, which was also left behind, was not in action: they could not be due, that is to say, to discharges of either of the two kinds of electricity then known; and having arrived at this point, he jumped from it to the conclusion, that the limbs themselves must have an electricity of their own, and that the contractions were brought about by discharges of this electricity. It never occurred to him to doubt that electricity was the agent at work in causing those contractions: and, in short, he did not hesitate to conclude, not only that the contractions were in themselves abundant proof of the existence of animal electricity, but also that the muscular fibres are charged during rest as Leyden jars are charged, that muscular contraction is the sign and effect of the discharge of this charge, the discharge, in one way or another, being brought about by an electrical action of the nerves upon the muscles.

From this time until the day of his death, Galvani went on performing experiment after experiment, sacrificing hecatombs of frogs, and never wavering in his belief in the existence of animal electricity, or in the conclusion he had come to respecting the action of this electricity in vital motion: but during his lifetime he was destined to be foiled in his hopes to bring others to the same mind with himself, and that too by a weapon which lay hid in one of his own experiments. The experiment in question was one in which a galvanoscopic frog was thrown into a state of momentary contraction by placing a conducting arc, of which one half was silver and the other half copper, between the lumbar nerves and the crural muscles[1]. Galvani, as was his wont, explained these contractions by supposing that the conducting arc had served to discharge animal electricity, and that the contractions were the result of the discharge. Volta, on the other hand, was of opinion that the electricity producing these contractions originated in certain reactions between the silver and copper portions of the conducting arc; and he was not shaken in this view by what he did afterwards, for, wishing to confirm it, he began a series of investigations which ended in the discovery of the voltaic pile and battery—

[1] The *galvanoscopic frog* was prepared from the hinder half of the animal, by stripping off the skin, and cutting away all the parts between the thighs and the fragment of the spine, except the principal nerves.

a discovery which filled all minds with wonder, and for a long time afterwards diverted attention altogether from the consideration of the claims of animal electricity. In the meantime, however, while Volta was demonstrating the existence of that electricity which originates in the reaction of heterogeneous bodies, and which is now known as voltaic electricity, Galvani continued his search after animal electricity, and made many important discoveries as he went along. He discovered, among other things, that a galvanoscopic frog would contract without the help of a conducting arc composed of heterogeneous metals. He discovered not only that these contractions would happen when this arc was composed of a single metal, but also that an arc composed of muscle or nerve would answer the same purpose as the metallic arc. He also discovered that the limb of a galvanoscopic frog, of which the nerve had been divided high up in the loins, would contract at the moment when the end of the nerve below the line of division was brought down and made to touch a part of the trunk of the same nerve. At last, indeed, he hit upon an experiment in which he seemed to have to do with an electricity other than that arising from the reaction of heterogeneous bodies—an electricity which must belong to the animal tissues themselves. He did much, but he did not do enough to win the battle in which he was engaged, for Volta still kept his position, denying the existence of animal electricity, and maintaining that the electricity which produced the contractions in the galvanoscopic frogs was always due to electricity arising in the reaction of heterogeneous bodies of one kind or other—silver and copper, metal and organic tissue, muscle and nerve, nerve in one state with nerve in another, as the case might be[1].

In 1799, Humboldt took up the question at issue between Galvani and Volta, and published a work[2] in which he shews by many new and curious experiments that there was error on both sides—that Volta was wrong in ignoring altogether the influence of animal electricity in Galvani's experiments, and that Galvani was not less wrong in recognising nothing but this influence. He, himself, as is proved in the extract already given, was a firm believer in animal electricity; but he failed to

[1] *Ann. de Chim.*, T. XXIII. p. 276 and 301. [2] *Op. cit.*

supply reasons for this belief which can be regarded as thoroughly satisfactory at the present day. Still, he did something in this direction by making out—first, that the agent assumed to exist, and to be animal electricity, has this in common with electricity, that its action is permitted by conductors and prevented by nonconductors; and, secondly, that it is not to be confounded with voltaic electricity, because the action, which is permitted by conductors, is possible across a gap in the circuit which would allow the passage of frictional electricity, but which would altogether prevent that of voltaic electricity—that is to say, allow electricity of high tension to pass, but not electricity of low tension. What Humbolt did, in fact, was to increase the probabilities of the existence of animal electricity not a little, and at the same time to make it appear that this electricity would prove to be of higher tension than voltaic electricity under ordinary circumstances.

In 1803, Aldini, Galvani's nephew[1], published an account of certain experiments which furnish further evidence in favour of the existence of animal electricity, by shewing that living animal tissues are capable of giving rise to attractions and repulsions which seem to be no other than electrical attractions and repulsions. "I held," he says, "the muscles of a prepared frog in one of my hands, moistened with salt and water, and brought a finger of the other hand, well moistened in the same way, near to the crural nerves. When the frog possessed a great deal of vitality, the crural nerves gradually approached my hand, and strong contractions took place at the moment of contact." And again :—"Being desirous to render this phenomenon more evident, I formed the arc by applying one of my hands to the spinal marrow of a warm-blooded animal, while I held the frog in such a manner that its crural nerves were brought very near to the abdominal muscle. By this arrangement the attraction of the nerves of the frog became very evident."

About this time, however, the discovery of the voltaic battery had given the victory to the opinions of Volta—a vic-

[1] "Account of the late Improvements in Galvanism, with a series of curious and interesting experiments performed before the Commissioners of the French National Institute, and repeated in the Anatomical Theatres of London, &c." 4to. London, 1803.

tory so complete that nothing more was heard about animal electricity for the next thirty years.

In 1827, Nobili[1] brought back the subject of animal electricity to the thoughts of physiologists by discovering an electric current in the frog. He made this discovery by means of the very sensitive galvanometer which he himself had invented a short time previously—an instrument which, as perfected by M. Du Bois-Reymond and others, by Sir William Thomson more especially, ought to be as prominent an object as the microscope in the laboratory of every physiologist. Immersing each end of the coil of the instrument in a vessel containing either simple water or brine, and completing the circuit between the two vessels with a galvanoscopic frog—the fragment of the spine being immersed in one vessel, and the paws in the other—he found that there was a current in the frog from the feet upwards, which current would cause a considerable permanent deflection of the needle—to 30° or more if brine were used, to 10°, or thereabouts, if water were substituted for brine. Nobili supposed that this current was peculiar to the frog, and in this he erred; but he did, nevertheless, a great thing, for, by this experiment, he furnished, perhaps, the first unequivocal proof of the real existence of animal electricity.

Twelve or thirteen years later, Matteucci published an essay[2] which, as M. De la Rive says[3], "restored to animal electricity the place which it ought to occupy in electrical and physiological phenomena." This essay, moreover, had a great indirect influence upon the fortunes of animal electricity, for M. Du Bois-Reymond, as he himself tells us, was led to undertake the investigations which have made his name famous in this department of physiology by the inspiration arising from its perusal.

The joint labours of MM. Matteucci and Du Bois-Reymond have left no room for entertaining any doubt as to the reality of animal electricity. This will appear sufficiently in the sequel, when many of the experiments which furnish the demonstration will have to be referred to particularly. In the

[1] *Bibl. Univ.*, 1828, T. XXXVII., p. 10.
[2] *Traité des Phénomènes Electro-physiologiques des Animaux.* Paris. 1844.
[3] *A Treatise on Electricity, in theory and practice.* Translated by C. V. Walker. 8vo. Longman. 1853-1858.

meantime, it may be said that Matteucci has demonstrated in the most unequivocal manner that animal electricity is capable of decomposing iodide of potassium, and of giving "signes de tension avec un condensateur délicat[1]," as well as of producing movement in the needle of the galvanometer; and not only so, but also—a fact, the discovery of which will always give Matteucci a place in the very foremost rank of physiological discoverers—that muscular contraction is accompanied by an electrical discharge analogous to that of the Torpedo. And as for M. Du Bois-Reymond[2] it may be said that he has demonstrated most conclusively that there are electrical currents in nerve—in brain, spinal cord, and other great nerve-centres, in sensory, motor, and mixed nerves, in the minutest fragment as well as in masses of considerable size,—that the electrical current of muscle, which had been already discovered by Matteucci, may be traced from the entire muscle to the single primitive fasciculus,—that Nobili's "frog-current," instead of being peculiar to the frog, is nothing more than the outflowing of the currents from the muscles and nerves,—that the law of the current of the muscle in the frog is the same as that of the current of muscle in man, rabbits, guinea-pigs and mice, in pigeons and sparrows, in tortoises, lizards, adders, glow-worms, toads, tadpoles, and salamanders, in tench, in freshwater crabs, in earthworms—in creatures belonging to every department of the animal kingdom,—that the law of the current in muscle agrees in every particular with the law of the current in nerve, and also with that of the feeble currents which are met with in tendon and other living tissues,—and that there are sundry changes in the current of muscle and nerve under certain circumstances, as during muscular contraction, during nervous action, under the influence of continuous and interrupted galvanic currents, and so on, which changes, as I shall hope to shew in the sequel, are of fundamental importance in clearing up much that would otherwise be impenetrable darkness in the physiology of muscular action and sensation.

Before the discovery of the galvanometer the attention of those who cared to meddle in these matters was directed

[1] *Cours d'Electro-Physiologie.* Paris. 1858.
[2] *Untersuchungen über thierische Electricität.* Berlin. 1849, 1853.

exclusively to the static phenomena of animal electricity. Then the only definite electrical ideas were, charge on the one hand, and discharge on the other. After the discovery of the galvanometer, the original point of view was abandoned altogether, or almost so, and the attention diverted from the static to the current phenomena of electricity. And herein, as I believe, was an unmixed misfortune. As I take it, indeed, it is necessary to go back to the standpoint occupied by Galvani and Humboldt, and to work with the electrometer rather than with the galvanometer; and this conviction has now so much gained upon me, that I am disposed to regard the New Quadrant Electrometer of Sir William Thomson—the instrument which for the first time makes it possible to arrive at an accurate knowledge of the statical aspects of animal electricity—as an instrument which is, to say the least, quite as indispensable as the galvanometer itself to those who would do the work in question. Already, indeed, as it will appear in due time, this instrument has supplied proof of the existence of a definite charge of electricity in nerve and muscle during rest, and of the discharge of this charge when this state of rest is changed for that of action, as well as of other facts without which, as I believe, it would be impossible to gain any real insight into the mystery of vital motion.

And thus, by the fact of the existence of animal electricity being now established beyond question, the way is more prepared than it was in the days of Galvani for the adoption of any view of vital motion in which animal electricity has to serve as the basis.

At the commencement of these introductory remarks I have told how I came to doubt the truth of the view of vital motion at present in favour, and to adopt in its stead a view which is substantially that of Galvani. I did not then know that Galvani had anticipated me; neither did I know that others were in the same predicament, about whose labours it still remains for me to say a few words.

The name to be mentioned first in order here is that of Dr West, of Alford, in Lincolnshire. As early as 1832[1], in some

[1] "On the Influence of the Nerves over Muscular Contractility," *London Medical and Surgical Journal*, edited by Michael Ryan, M.D. Vol. I. 1832.

remarks upon the influence of the nerves upon muscular contractility, this writer maintains, "that the nervous influence which is present in relaxed muscular fibre is the only influence which the nerves of volition possess over that tissue; that its office there is to restrain or control the tendency to contract which is inherent in the muscle; and that contraction can only take place when by an act of the will this influence is suspended, the muscle being then left to act according to its own innate properties;" . . . and again, "that nervous influence is imparted to muscular fibre for the purpose of restraining its contraction, and that the action of the will, and of all other disposers to contraction, is simply to withdraw for a while this influence, so as to allow this peculiar property of muscular fibre to shew itself." The co-existence of spasmodic action with nervous debility, the efficacy of stimulants as antispasmodics, and the postponement of rigor mortis until all traces of nervous action have disappeared, are the principal facts which are adduced in support of the probability of this theory.

Very shortly after the publication of these remarks, a similar idea appears to have been hinted at by Sir Charles Bell in a lecture at the Royal College of Surgeons in London, for, after premising that the question could never be settled, the lecturer said, "that *relaxation* might be the act, and not contraction, and that physiologists, in studying the subject, had too much neglected the consideration of the mode by which relaxation is effected." This remark is preserved by Dr West in the essay to which reference has just been made.

Six years later, in a chapter of his classical work on comparative anatomy[1], Professor Dugès, of Montpellier, argues with much clearness that all organic tissues are the seat of two opposite movements—expansion and contraction, and that contraction, which is in no sense peculiar to muscle, is nothing more than the cessation of expansion"—"la contraction musculaire ne consiste que dans l'annihilation de l'expansion." The muscle is supposed to contract in virtue of its elasticity, just as a piece of caoutchouc might contract when set free from a previous state of extension; and an analogy is hinted at between the expanded

[1] *Traité de Physiologie comparée de l'Homme et des Animaux.* 8vo. Montpellier and Paris. 1838.

state of the muscle and the fluid state of the fibrine of the blood, and between rigor mortis and the coagulated state of this fibrine. Analogous in its effects to electricity, the vital agent is supposed to accumulate in the muscles, and to produce expansion by causing the muscular molecules to repel each other; and contraction is supposed to be brought about either by the sudden discharge (as in ordinary contraction) or by the gradual dying out (as in rigor mortis) of the vital agent. And, further, it is supposed that the rhythmical movements of muscle are caused by successive discharges of the vital agent, which discharges are brought about whenever this agent acquires a certain degree of tension; and that the cramps of cholera, or the spasms of tetanus or hysteria, are consequent upon the development of the vital agent being for the time suspended.

More recently still, namely in 1847, Professor Matteucci communicated a paper to the Parisian Academy of Sciences[1] upon the influence of the nervous *fluid* in muscular action, in which he writes:—"Ce fluide développé principalement dans les muscles, s'y répand, et, doué d'une force répulsive entre ses parties, comme le fluide électrique, il tient les éléments de la fibre musculaire dans un état de répulsion analogue à celui présenté par les corps électrisés. Quand ce fluide nerveux cesse d'être libre dans le muscle, les éléments de la fibre musculaire s'attirent entre eux, comme on le voit arriver dans la roideur cadavérique. . . . . Suivant la quantité de ce fluide qui cesse d'être libre dans le muscle, la contraction est plus ou moins forte." Professor Matteucci appears to have framed this hypothesis, partly, in consequence of certain considerations which seemed to shew that the phenomenon of "induced contraction" was owing to the *discharge* of electricity in the muscle in which the "inducing contraction" was manifested—an idea originating with M. Becquerel—and, partly, in consequence of the analogy which he himself had found to exist between the law of contraction in muscle and the law of the discharge in electrical fishes; but he does not appear to have attached much importance to the hypothesis. Indeed, his own comment at the time is—"j'ai presque honte d'avoir eu la hardiesse de communiquer à l'Académie des idées si vagues,

[1] *Comptes Rendus.* March 17th, 1847.

et apparemment si peu fondées, et contre lesquelles on pourrait faire bien des objections, mais je pense que, parmi les théories physiques les mieux fondées aujourd'hui, il en existe qui ont débuté de cette manière, et il est certain que des hypothèses, aussi peu fondées que celles-ci, ont quelquefois pu produire ensuite des découvertes remarquables."

Next in order, and almost contemporaneously with the date of my own first publication on the subject, Professor Engel, of Vienna, wrote[1]:—"So hat der Nerve die Aufgabe, nicht die Zusammenziehungen des Muskels zu veranlassen, sondern den Zusammenziehungen bis auf einen geringen Grad entgegenzuwirken. Im lebenden Organismus, in welchem Ruhe etwas unmögliches ist, ist auch ein ruhender Muskel eben so wohl wie ein ruhender Nerv undenkbar, der Muskel in seinem beständigen Streben, sich zusammenzuziehen, wird vom Nerven daran verhindert, im Nerven macht sich das fortwährende Streben kund, die Zusammenziehung des Muskels auf ein gerechtes Mass zurückzuführen; das Ergebniss dieser zwei einander entgegengesetzten Eigenschaften des Nervens und des Muskels ist das, was man gemeinhin Zustand der Ruhe, Zustand des Gleichgewichtes, oder an Muskeln auch Tonicität nennt. Das Verlassen dieses Gleichgewichts ist die Bewegung einerseits, die Lähmung andererseits. Die Bewegung wird aber erzeugt, indem entweder der Einfluss des Nervens auf den Muskel herabgesetzt wird, oder indem die Contractionskraft des Muskels unmittelbar gesteigert wird. Lähmung des Muskels findet sich gleichfalls entweder durch unmittelbare Vernichtung der Contractionskraft des Muskels oder durch eine übermässig gesteigerte Einwirkung des motorischen Nervens auf den Muskel. Sollen daher abwechselnde Muskelcontractionen zu Stande kommen, so ist die Gegenwart des lebendigen Nervens im Muskel unerlässlich, und auch bei unmittelbaren Muskelreizen können abwechselnde Zusammenziehungen nur erfolgen, so lange noch die Nerven lebensfähig sind; hört letzteres auf, so zieht sich der Muskel ohne Hinderniss zusammen. Diesen Zustand nennen wir die Todtenstarre." The chief grounds for this opinion are, first, certain original experiments, some of

[1] "Ueber Muskelreizbarkeit," *Zeitschrift der Kais. Kön. Gesellsch. der Aertze zu Wien*, Erster Band, pp. 205—219, and pp. 252—270. 1849.

them very remarkable, which afford additional proof that the muscles of frogs are more prone to contract when they are cut off from the influence of the great nervous centres, secondly, the frequent spontaneous occurrence of cramps and other forms of excessive spasmodic contraction in paralysed parts, and, thirdly, the supervention of the permanent contraction of rigor mortis when all signs of nervous irritability are completely extinguished.

And, last of all, I find Professor Stannius, of Rostock[1], arriving at the conclusion:—"dass es eine wesentliche Aufgabe der sogenannten motorischen oder Muskelnerven sei, die natürliche Elasticitätsgrösse der Muskelfasern herabzusetzen und ihre Elasticität vollkommener zu machen; dass anscheinende Ruhe des Muskels, zum Beispiele, während des Schlafes, das Stadium solchen regen, den Muskel zu seinen Aufgaben wieder befähigenden Nerveneinflusses anzeige; dass active Muskelzusammenziehung einen geregelten und begrenzten momentanen Nachlass des Nerveneinflusses auf den Muskel bezeichne; dass endlich die Nachweisung einer Muskelreizbarkeit, in der üblichen Auffassungsweise, ein durchaus vergebliches Bemühen sei." M. Stannius was led to this conclusion by certain original experiments, in which he found blood to have the power of relaxing rigor mortis and restoring muscular irritability, and these experiments are advanced in evidence. Reference is also made to arguments, to be brought forward on another occasion, which will prove—"dass diese Anschauungsweise, so paradox sie immer auf den ersten Anblick sich anlassen mag, mit unserem thatsächlichen Wissen über Nerven- und Muskelthätigkeit keineswegs im Widerspruch steht." The essay from which these quotations are taken was published towards the end of 1852—about two years after the date of my own first publication on the subject.

I do not stand alone, then, in thinking that a great change is necessary in the theory of *muscular motion*—a change amounting to no less than a complete revolution; and I am glad that it is so, for thus supported, I am the more bold to challenge attention to the facts and arguments which will be advanced on a future occasion.

[1] "Untersuchungen über Leistungsfähigkeit der Muskeln und Todtenstarre," *Vierordt's Archiv für Physiol. Heilkunde.* Stuttgart, 1 Heft, p. 22, 1852.

DISSECTION OF A LAMB WITH FISSURE OF THE STERNUM AND TRANSPOSITION OF THE ORIGIN OF THE RIGHT SUBCLAVIAN ARTERY.

By LESLIE OGILVIE and CHARLES W. CATHCART, *Students of Medicine, University of Edinburgh.*

WE owe to Professor Turner the opportunity of dissecting a ewe lamb, in which a median ventral fissure was conjoined with other malformations. The specimen was presented by Dr Mac Watt of Dunse.

On the ventral surface of the body there existed a distinct mesial cleft, which extended from the root of the neck to the place of exit of the umbilical cord two and a half inches in front of the nipples. This cleft penetrated not only through the skin and immediately subjacent structures, but also through the sternum, which was divided into two parts, to each of which were attached the appertaining rib-cartilages. The linea alba in front of the place of exit of the cord was also divided, but behind the cord the anterior wall of the abdomen was complete. From this deficiency in the thoracic and abdominal walls, the heart enclosed in the pericardium, the liver, the stomach, the small intestine the cœcum and greater part of large intestine, were exposed. The apex of the heart, the greater part of the liver, the abomasum and psalterium, the small intestine, the cæcum and greater part of the large intestine, protruded through the fissure. Owing to the longitudinal division of the sternum, and the increased space between the ribs of the two sides, a deep fissure, at the bottom of which could be seen the posterior vena cava, existed in the muscular and tendinous substance of the diaphragm.

Tracing the peritoneal membrane forwards from the anterior surface of the liver, it was found that the falciform ligament passed to the free border of the left half of the diaphragm, where it split into two layers, one of which was prolonged over the posterior surface of that half, whilst the other passed to the posterior surface of the right half, covering in its course the

posterior vena cava, and bridging across the fissure or interval between the two parts of the muscle. In bridging across this interval, just after leaving the margin of the left half of the diaphragm, the peritoneal membrane came in relation to the posterior part of the left pleura for about a quarter of an inch, but did not unite with it. It next united with the posterior surface of the pericardium, where this membrane would have covered the anterior surface of the diaphragm, had that muscle been complete. It then united with the posterior part of the right pleura, after which it passed to the posterior surface of the right half of the diaphragm, the pleura, as usual, covering its anterior surface. After lining what existed of the diaphragm on each side, the peritoneum passed over the abdominal walls, and ended in a defined margin at the border of the great cleft.

The pericardium was torn on its ventral surface, so as to expose part of the heart, but it probably had formed a complete sac. Its posterior surface was blended with the inter-diaphragmatic portion of the peritoneum which was not in relation to the pleuræ. Its sides were in apposition with the pleural membranes, but the ventral surface was free. The bag of the pericardium was very thin and transparent, smooth and serous on its inner surface, and was reflected on to the exterior of the heart in the usual manner.

Each pleura existed as a shut sac attached below to the corresponding half of the sternum. Owing, however, to the fissure, the inter-pleural space was abnormally large. Upon opening into the pleural cavities the left lung was found collapsed, while the right was partially inflated. The posterior part of the right pleura was in relation to a greater extent of surface of the peritoneum than the left, for although the origin of the diaphragm on both sides reached as high as the eighth rib, on the left side it also had an attachment to the xiphoid cartilage. Thus the gap left in the posterior thoracic wall by the fissure in the diaphragm was occupied by a thin twofold membrane composed of peritoneum and pleura on each side, and of peritoneum and pericardium in the middle.

Blood-vessels.—The aorta divided in the normal manner into anterior and posterior aortæ, the latter of which arched over the

root of the left lung. The anterior aorta passed forwards for about half an inch on the ventral aspect of the trachea and to its left side, and divided into two branches. One of these, which formed the common stem for the two carotids, was prolonged in the same direction for about half an inch before bifurcating. The other passed forwards and to the left, to be continued as the axillary artery, having first given off the vertebral, the posterior cervical and dorsal branches. The artery which corresponded to the right subclavian, instead of being a branch of the anterior aorta, arose from the posterior aorta to the left of the œsophagus, and half an inch in front of the root of the left lung. It then passed to the right side between the œsophagus and second dorsal vertebra, from which it was separated by the origins of the prevertebral muscles. It passed forwards out of the apex of the thoracic cavity, and was continued to the right anterior extremity in the normal manner.

It will be seen that the arrangement of these vessels in this lamb differs from the normal arrangement in ruminants:—(1) in the right division of the anterior aorta not being continued as the right axillary, but passing forwards to form a common stem for the two carotids; (2) in the right subclavian—or what corresponds to it—arising from the posterior aorta. An explanation of such deviations in the origins of the vessels is obtained by reference to the developmental changes which take place in the vascular arches of the embryo, as was described in a detailed manner, and with ample reference to illustrative cases, by Prof. Turner, in a memoir published in the *Medico-Chirurgical Review* for 1862. In this paper numerous instances are mentioned where, in the human subject, the right subclavian arose from the arch of the aorta to the left of the origins of the right carotid, left carotid and left subclavian, and passed, as in this lamb, behind the trachea and œsophagus to the right side. The explanation given is that the fourth right vascular arch becoming atrophied does not form the right subclavian, and consequently that the right aortic root, instead of disappearing, as is the case in the normal development of the vessels, remains pervious, so as to convey the blood to the right upper limb. The part of the artery, therefore, which, in this lamb, lay behind the œsophagus, was

really the persistent pervious right aortic root, and its point of junction with the left aortic root, opposite the second dorsal vertebra, was, without doubt, the point at which the two aortic roots had united.

Bones.—The cervical vertebræ displayed no peculiarity except that the axis and third vertebræ were fused together. The fusion was more complete at the articular processes, and, on the ventral surface of the bodies, distinct traces of separation were seen between the neural arches, and above and below the articular processes. The two fused bones were displaced slightly to the left side, and the right halves of the bodies were more closely pressed together than the left halves. The articular processes also on the right side were very feebly developed, while on the left they seemed normal.

The dorsal vertebræ, thirteen in number, were the most irregular of the true vertebræ. A well-marked lateral curvature of the spinal column to the right existed between the first and eleventh vertebræ, and was greatest between the fourth and seventh. Moreover, this part of the spinal column was twisted on itself so as to depress the left sides of the bodies and raise the right. The centres of the inferior aspect of the bodies were displaced to the right side of the space between the ribs. The second, third, and fourth dorsal vertebræ were fused together even more closely than the second and third cervical. Their spines were blended into one deep blade, the top of which preserved the same dorsal curvature as the other spines. The relative position of the three fused bones made it appear as if they had been fused while the spinal column was straight, and that after their union their posterior part had been displaced to the right, which had caused the vertebræ behind to share in the curvature. The lumbar vertebræ were seven in number and quite regular.

The sacrum, somewhat more developed on the right side than on the left, was twisted at the coccygeal end so as slightly to raise the right border.

There were thirteen pairs of ribs, of which eight were true and five false on each side. From the third to the eleventh they were more or less irregular on both sides, but more especially from the fourth to the seventh. The shafts of these

ribs on the left side were straight instead of being curved outwards, and this caused the thoracic cavity to be narrowed from side to side. The right ribs were all inclined backwards at a more acute angle to the axis of the spinal column than were the left. The latter however all projected to a lower level than the former owing to the twisting of the spine already referred to.

The lateral curvature of the spine, the concavity of which was to the left and greatest between the fourth and seventh vertebræ, caused the heads of the corresponding ribs to be approximated, and the shafts to be thrust together so closely as to prevent growth. Accordingly they were much thinner in their central parts.

On the right side the articular processes of the vertebræ, being placed on the circumference of part of a circle, were thrown further apart than usual. Hence a line drawn from the articular facet on any transverse process to the facet on the side of the vertebra next anterior was made to slope more acutely backwards than it would otherwise have done. From this arrangement three results followed. 1. The ribs of this side were brought closer to the spinal column than usual. 2. The inclination of the shafts downwards was more acutely backwards than on the other side. 3. The broad surfaces of the ribs looked anteriorly and posteriorly as well as outwards and inwards.

The two halves of the sternum were not absolutely symmetrical. Owing to the difference in the obliquity of the ribs of the two sides, the right half was projected further forward than the left, and would have been on a lower plane, had it not been that the cartilages of the true ribs on the left side passed horizontally forwards and inwards, whilst those on the right passed forwards, downwards, and only very slightly inwards.

The centres of ossification in each half of the sternum were the following :—

On the left side six, all well developed. The first between the cartilages of the first and second ribs was normal in shape. The second was between the second and third costal cartilages. In common with the remaining four, it was of the shape of a quarter of a sphere with the two flat surfaces looking respectively inwards and downwards. The third was between the

third and fourth costal cartilages. The fourth was between the fourth costal cartilage and a point above the level of the insertion of the fifth. The fifth, from the insertion of the fifth costal cartilage to above that of the sixth. The sixth from the insertion of the sixth costal cartilage to above that of the seventh and eighth conjoined.

On the right side were five centres, all well developed except the first. The first between the first and second costal cartilages was a mere speck of bony matter about the size of a pin's head. The second was between the second and third costal cartilages, and with the three following was of the quarter sphere shape. The third was between the third and fourth costal cartilages. The fourth was between the fourth and fifth costal cartilages. The fifth, larger than the others, was placed below the fifth costal cartilage, opposite to the sixth, and anterior to the conjoined seventh and eighth.

The halves of the xiphoid cartilage were well developed, and continuous with each half of the sternum. The left part was larger than the right, and had an elongated well-developed centre of ossification close to the posterior part of the sternum.

According to Geoffrey St Hilaire[1] median fissures generally are to be accounted for by a want of development, *i.e.* by two halves, which should have been prolonged to meet each other, having been arrested in their growth so as to fall short of their line of junction, and the only reasonable explanation of the cleft in this lamb's sternum that we can give is that at a very early period in its fœtal life the ventral laminæ had failed to unite along the mesial line. Why in this case a stoppage of the ventral union should have occurred we cannot say. That an abdominal cleft should be connected with the opening for the exit of the umbilical cord is only natural, but this gives us no clue to the persistence of the fissure anterior to the umbilicus. It is possible that the curvature of the spine and consequent distortion of the ribs may have followed from the ventral laminæ not having united; for the non-union of the ribs in a common sternum would render these bones less able to resist any distorting influences which may have been brought to bear upon them.

[1] *Histoire des Anomalies*, &c. 1832. Vol. I. p. 596.

ON THE SENSE OF ROTATION AND THE ANATOMY AND PHYSIOLOGY OF THE SEMICIRCULAR CANALS OF THE INTERNAL EAR. By PROF. A. CRUM BROWN, M.D. *University of Edinburgh.*

FOR some time I have been convinced that we possess a sense of Rotation quite distinct from all our other senses. By means of this sense we are able to determine—1st, the axis about which rotation of the head takes place; 2nd, the direction of the rotation; and 3rd, its rate.

In ordinary circumstances we do not wholly depend upon this sense for such information. Sight, hearing, touch, and the muscular sense assist us in determining the direction and amount of our motions of rotation, as well as of those of translation; but if we purposely deprive ourselves of such aids we find that we can still determine with considerable accuracy the axis, the direction, and the rate of rotation. The experiments that I have made with the view of determining this point were conducted as follows: a stool was placed on the centre of a table capable of rotating smoothly about a vertical axis; upon this the experimenter sat, his eyes being closed and bandaged; an assistant then turned the table as smoothly as possible through an angle of the sense and extent of which the experimenter had not been informed. It was found that, with moderate speed, and when not more than one or two complete turns were made at once, the experimenter could form a tolerably accurate judgment of the angle through which he had been turned. By placing the head in various positions it was possible to make the vertical axis coincide with any straight line in the head. It was found that the accuracy of the sense was not the same for each position of the axis in the head, and further, that the minimum perceptible angular rate of rotation varied also with the position of the axis. It was also found that considerable differences of accuracy exist in different individuals.

The sense of rotation is, like other senses, subject to illusions, rotation being perceived where none takes place. Vertigo or giddiness is a phenomenon of this kind.

When, in the experiments just mentioned, rotation at a uniform angular rate is kept up for some time, the rate appears to the experimenter to be gradually diminishing, and to cease altogether after a time, varying with the position of the head, and different with different individuals; if the rotation be then stopped, he experiences the sensation of rotation about the same axis in the opposite direction. If the position of the head be changed after the prolonged rotation has been made, the position of the axis of the apparent rotation is changed, retaining always the same position relatively to the head as was occupied by the axis of the real rotation. The readiness with which this *complementary apparent rotation* is produced is not the same for each axis. In such experiments, as long as the eyes are shut, and the axis of rotation kept vertical, a sensation of giddiness is not experienced. That sensation appears to be caused by the discordance between the testimony of the sense of rotation, and that of some other sense. Thus if I experience a sensation of rotation, it makes no difference to my mind whether that sensation corresponds to a real rotation or not, as long as I have no means of ascertaining independently the existence or non-existence of the real rotation. I am in that position as long as my head is fixed and my eyes shut. But if, while the *complementary apparent rotation* is felt, I open my eyes, I still feel that I am being turned round, but at the same time I *see* that external bodies retain their position relatively to me—if I am turning round so are they—and this produces at once a feeling of insecurity or giddiness. Similarly this giddy or insecure feeling is produced, if, while the *complementary apparent rotation* is felt, the head be moved so that the axis of this rotation is no longer vertical.

The sense of Rotation, being a special sense, must necessarily have a special peripheral organ physically constituted so as to be affected by rotation, a sensory nerve, and a central organ. The structure of the semicircular canals of the internal ear is such as to fit them to act as such a peripheral organ, and the experiments of Flourens and of Goltz support this

view. The bony canals are filled with liquid, in which float loose connective tissue and the membranous canals with the contained endolymph. Rotation of the head about an axis at right angles to the plane of a canal will then produce, on account of the inertia of the liquid, &c., motion of the contents relatively to the walls of the canal, and this may be expected to irritate the terminations of the nerves in the ampulla. If the rotation be continued at a uniform rate, fluid friction, friction of the endolymph against the membranous canal, and of the perilymph against the membranous canal and the periosteum, will gradually diminish this relative motion, which will at last cease. We should therefore expect, as we have seen to be the case, that continued uniform rotation should be perceived less and less strongly, and that the sensation should at last die away altogether. The time required for this equalisation of the motion of the canal and its contents will depend upon the rate of rotation and upon the dimensions of the canal and the amount of attachment of the membranous canal to the periosteum. These latter conditions are not the same in the three canals, and therefore we ought to find, as we do, that the rate at which the sense of rotation dies away is not the same for different positions of the head. Again, if the uniform rotation is stopped, the contents of the canal will continue to move on, thus causing an apparent rotation in a direction the reverse of that of the original rotation, and this also will die away owing to friction.

As the three canals are in planes nearly at right angles to one another, rotation about any axis can be resolved into rotations, each of which will produce the effect described above upon one of the canals, and thus any rotation will have its appropriate sensation.

So far then this view of the function of the semicircular canals seems to explain the phenomena of the sense of rotation, and I find that an explanation almost identical with this was given by Professor Mach, of Prague, and by Dr Breuer, of Vienna, shortly before I communicated the substance of this paper, as a preliminary note, to the Royal Society of Edinburgh[1].

[1] *Proceedings*, 19th January, 1874.

But this explanation is not sufficient. As far as we know, a nerve current can vary only in intensity and not in kind, so that, if irritated at all, whether by right-handed or by left-handed rotation, the nerve would convey the same message to the central organ. The solution of this difficulty which I proposed is as follows:—Each canal has an ampulla at one end only, and there is thus a physical difference between rotation with the ampulla first, and rotation with the ampulla last, and we can easily suppose the action to be such that only one of these rotations (say that with the ampulla first, in which case, of course, there is a flow from the ampulla into the canal) will affect the nerve terminations at all[1]. One canal can therefore, on this supposition, be affected by, and transmit the sensation of rotation *about one axis in one direction* only, and for complete perception of rotation in any direction about any axis *six* semicircular canals are required, in three pairs, each pair having its two canals parallel (or in the same plane) and with their ampullæ turned opposite ways. Each pair would thus be sensitive to any rotation about a line at right angles to its plane or planes, the one canal being influenced by rotation in the one direction, the other by rotation in the opposite direction.

Now we have six semicircular canals, three in the one ear and three in the other, and I find in all the animals that I have examined that the exterior canal of one ear is very nearly in the same plane as that of the other; while the superior canal of one ear is nearly parallel to the posterior canal of the other.

The three axes are therefore—1st, a vertical[2] axis at right angles to the plane of the exterior canals; 2nd, an axis which may be roughly defined as passing (in the human subject) through the left eye and the right mastoid process at right angles to the planes of the right superior and the left posterior canal, and 3rd, an axis passing through the right eye and the left mastoid process at right angles to the right posterior and left superior canals.

In different animals there are great differences in relative

[1] In the preliminary note above referred to, I described a way in which this might be supposed to take place.

[2] In the human subject this axis is not quite vertical when the head is held in its usual position; it becomes so when the face is inclined slightly downwards.

size and position of the canals, but the relation just mentioned appears to exist in all cases. This relation may be most simply stated thus. In each ear there is one canal (the exterior) in a plane at right angles to the mesial plane, and two other canals (the superior and the posterior) in planes equally inclined to the mesial plane. In no other way is it possible to harmonize the bilateral symmetry of the two ears with the condition that each of the three axes shall have two *oppositely turned* canals in planes at right angles to it.

EFFECT OF WARMTH IN PREVENTING DEATH FROM CHLORAL. By T. LAUDER BRUNTON, M.D., Sc.D. Edin. *Casualty Physician and Lecturer on Materia Medica at St Bartholomew's Hospital.*

SINCE chloral was first brought into notice, and its action investigated by Liebreich, it has been made the subject of numerous experiments, and has not only proved a most useful medicine, but a valuable aid to physiological research. During the stay made in this country by Professor Stricker four years ago he used chloral frequently as an anæsthetic while making some experiments with Dr Burdon Sanderson on the circulation in mammals. At his suggestion I made the following experiments as well as many others which it is quite unnecessary to give at length, as they simply confirm the observations of Liebreich and others. The general results were that the subcutaneous injection of a solution of chloral induced sleep, which was light and readily broken if the dose were small, but passed into coma if the dose were large. In dogs considerable restlessness was observed before sleep came on. The power of muscular co-ordination was affected in dogs before sopor was induced, so that they staggered and fell when attempting to walk either of their own accord or in obedience to a call. A similar loss of co-ordination was observed in rabbits, but in general they and guinea-pigs sat quietly after the administration of the chloral, and thus the motor affection was less perceptible in them than in dogs. In dogs the respiration occasionally became very rapid immediately after the subcutaneous injection of chloral, but it became slow after the animal began to exhibit symptoms of drowsiness. In rabbits and guinea-pigs the number of respirations was also diminished, but a preliminary acceleration was not observed in them. The pulse was not affected to the same extent as the respiration, and the heart always continued to beat after the respiratory movements had ceased. One of the most important phenomena,

and the one to which I wish to call particular attention at present, is the diminution of temperature which chloral induces, and the extraordinary effects of warmth in hastening recovery from its action, and preventing death from an overdose. The fall of temperature has been noticed by Liebreich and most other writers, but the effect of warmth applied to the animal's body has not, I think, received sufficient attention, although Dr Richardson has pointed out its usefulness in preventing death. The diminution of animal heat is partly due in all probability to greater loss from the surface caused by the vessels of the skin becoming much dilated under the influence of the drug, and allowing the blood to be cooled more readily by a low external temperature. It is partly due also to the diminished production of heat which cessation of muscular action always causes, whether it be induced by simply tying down an animal so as to prevent motion, or by the administration of curare or narcotics.

Professor Stricker having noticed that the animals on which he experimented often required a second dose of chloral to maintain anæsthesia, when they were wrapped in cotton wool so as to prevent loss of heat, and still more when they were laid in a warm place, I made the following experiments at his suggestion. They shew clearly that an animal wrapped in cotton wool may recover perfectly from a dose of chloral which is sufficient to kill it when exposed to the cooling action of the air (which in the laboratory was about 20° C.), and that recovery from the narcotic action is much quicker when the temperature is maintained in this way, and still more rapid when the animal is placed in a warm bath. If the temperature of the bath is too high the animal may die from excessive heat, as I have shewn in a former paper[1].

The bearing of these experiments on the treatment of persons suffering from an overdose of chloral is so obvious as hardly to require any observations from me. The patient should be put to bed, and the temperature of the body maintained by warm blankets and hot-water bottles to various parts of the body, and especially the cardiac region. Warmth over the

[1] "On the Effect of Temperature on the Mammalian Heart and on the Action of the Vagus." St Bartholomew's Hospital *Reports*, Vol. VII. 1871.

heart is an excellent stimulant to the circulation, which, like the respiration, is enfeebled by chloral, the heart according to Rajewsky being more or less paralyzed by the drug. If respiration threatens to fail it should be maintained artificially so as to allow time for the chloral to be excreted and the normal functions to be restored.

Expt. I. Into two guinea-pigs of nearly equal size ·6 cubic centimetre of a 50 per cent. solution of chloral (equal to about 5 grains or ·3 gramme of chloral) were injected subcutaneously.

	Temp. of animal.		
	No. 1.	No. 2.	
Before injection	103·8°F.	104·6°F.	
After injection 5 min.			No. 1 was almost motionless, and was rolled in cotton-wadding. No. 2 was left uncovered.
27 min.	99·7	101·5	
1 hr. 20 min.	96·	92·6	
1 hr. 40 min.	95·2	91·	
2 hr. 33 min.	94·3	about 80°	The graduation of the thermometer did not extend so low as 80°, and the temperature was calculated approximately, or guessed, by the height of the mercury.
2 hr. 57 min.	93·8		The legs of No. 2 are shivering.
4 hr.	87·3		
4 hr. 42 min.	88·4		No. 1 was now deprived of its covering. It shivered and grunted but lay in whatever position it was put, and was still completely narcotised.
5 hr. 27 min.	88·8		
5 hr. 42 min.			No. 1 awakened. No. 2 remained as before, motionless, except for the shivering.
5 hr. 52 min.	95·8		It did not recover, and died some little time afterwards.

Expt. II. Into two guinea-pigs, No. 1 weighing 655 grammes, and No. 2 weighing 670 grammes, 1·1 cub. cent. of 50 per cent. solution of chloral hydrate was injected.

	No. 1.		No. 2.		
	Temp.	Resp.	Temp.	Resp.	
	102·4°				
After injection 7 min.	101·8	89	101°		Both animals nearly narcotized. No. 1 put into a warm-air bath. No. 2 allowed to lie without cover.
1 hr. 53 min.			91·8	24	
2 hr. 7 min.	102·9	78			
2 hr. 20 min.	102·4	50			Kicks vigorously when pinched.
2 hr. 34 min.			abt. 87	12	Does not kick when pinched.
8 hr.	98·8			13	No. 1 is now awake.
24 hr.					Temperature of No. 2 cannot be taken or estimated, as it is so low that the mercury does not rise from the bulb of the thermometer. No respiration is visible, but occasionally the animal opens its mouth convulsively. No reflex. It was put in the warm bath, but respiration did not become re-established, and all signs of life shortly disappeared.

Expt. III. Into a guinea-pig (No. 1) weighing 272 grammes was injected ·65 cub. cent. of a 50 per cent. solution of hydrate of chloral. Into another (No. 2) weighing 330 grammes the same quantity of the solution was also injected.

	No. 1.		No. 2.		
	Temp.	Resp.	Temp.	Resp.	
At injection	100·9		100·8		
After injection 7 min.					No. 1 fast asleep. It was put into a warm-air bath. No. 2 not quite asleep.
12 min.					No. 2 nearly quite asleep. Lies as it is placed. Reflex movements are slight.
26 min.			93·1	46	
50 min.	100·7	62			
1 hr. 26 min.				28	Shivers very much with expiration, so that the respirations are difficult to count. Grunts slightly when the thermometer is introduced into the rectum.
2 hr. 4 min.	102°	104	about 89·5°	30	Both animals are chewing.
2 hr. 38 min.	101·8		88·5		No. 1 is awake. Though still somewhat sleepy it will no longer lie on its back. No. 2 cries when pinched. It was put into the warm bath.
4 hr. 6 min.			95·6		No. 2 is now awake.

In this Exp. (No. III.) the dose was small and guinea-pig No. 2 recovered, although it was not kept warm, but not till an hour and a half after No. 1, although the latter was the smaller animal, and the dose it received was therefore much greater in proportion to its size.

Expt. IV. Into each of three guinea-pigs 1·1 cubic centimetre of 50 per cent. solution of hydrate of chloral was injected subcutaneously. No. 1 weighed 640 grammes, No. 2, 670 grammes, and No. 3, 717 grammes.

	No. 1.		No. 2.		No. 3.		
	Temp.	Resp.	Temp.	Resp.	Temp.	Resp.	
	101·6		102·5		101·4		No. 1 was put at once into a hot-air bath after the injection.
After injection 19 min.							No. 2 was quite narcotised and was wrapped in cotton-wool.
9 min.							No. 3 was narcotised, and was left lying on table.
2 hr. 20 min.					85	17	
2 hr, 40 min.			97	36			
2 hr. 50 min.	104·4	74					
3 hrs.			95·3		83	20	Saliva runs from mouth.
	103·5	70					
5 hr. 30 min.					?	10	
5 hr. 50 min.			94·6				Begins to shew signs of reflex when pinched.
6 hrs.	100·6	76					
7 hr. 50 min.						8	
8 hr. 10 min.			93·2				Is beginning to awake.
8 hr. 20 min.	100·8						No. 1 is dead. It appeared to have had convulsions, for some of the cotton-wool lining the bath was caught in its teeth.
22 hrs.							No. 3 is still alive. On pinching one hind-foot he moves both it and the other one. Occasionally opens its mouth in a convulsive manner and paws with its feet while it lies on its side. It died a short while after.

In this experiment the animal No. 1 died in consequence of too high a temperature of the bath.

Expt. V. Into the flank of a guinea-pig (No. 1) weighing 392 grammes was injected ·75 cc. of a 50 per cent. solution of chloral, and into the axilla of another (No. 2) weighing 335 grammes ·9 cc. of the same solution.

	Temp. of animal.		
	No. 1.	No. 2.	
After injection 3 min.			No. 1 lies quite quiet. No. 2 put into a warm-air bath at 98·6°.
5 min.	98·2	98	
12 min.		98·3	No. 1 is dead. The heart beat after the respiration stopped. The respirations of No. 2 became very rapid and deep after it was put in the air-bath.
2 hrs. 26 min.		111·4	No. 2 was heard to give a grunt, and on taking it out almost immediately after it was found to be dead.

The dose was here either too large or the temperature of the bath rose too high.

Expt. VI. Into each of three guinea-pigs, No. 1 weighing 490 grammes, No. 2 weighing 425 grammes, and No. 3 weighing 415 grammes, 1·1 cubic centimetres of a 50 per cent. solution of chloral hydrate were injected subcutaneously.

	No. 1.		No. 2.		No. 3.		
	Temp.	Resp.	Temp.	Resp.	Temp.	Resp.	
A minute or two after injection	100	90	99·4	96	100	84	No. 2 and 3 quite narcotised.
After injection 5 min.							No. 1 quite narcotised. No. 1 left exposed. No. 2 rolled up in cotton wadding, and No. 3 put into a warm-air bath at 86°.
31 min.		26					No. 1 shews some reflex action of foot when it is pinched.
1 hr. 13 min.	abt. 87°	13					Respirations of No. 1 are very deep.
2 hr. 15 min.			96·8	36			
2 hr. 26 min.					105·4	90	
2 hr. 35 min.	?	8					
3 hr. 35 min.		4					Pulse of No. 1 is 80 per min.
3 hr. 56 min.			93·6	27			
4 hr. 7 min.					102·6	94	No. 3 grunts when pinched.
4 hr. 16 min.							No. 1 is dead.
6 hr. 45 min.			92·5	36			No. 3 is awaking.
6 hr. 56 min.					103·6		No. 3 is running about.
10 hrs.			85	34			
20 hr. 30 min.			98				No. 2 has just been delivered of a young one which is dead.
22 hrs.							No. 2 can run about. No. 2 seems quite well; eats heartily.

This experiment shews the effect of warmth in preventing death, and the rapidity of recovery when the animal is put in a warm bath, compared with that which it makes when encased in cotton-wool.

All these experiments were made in August, 1870, in the laboratory of my friend Dr Burdon Sanderson, and I gladly take this opportunity of returning him my most hearty thanks for the facilities and aid he afforded, and the kindness which he then and ever has shewn me.

THE SEPTUM ATRIORUM OF THE FROG AND THE RABBIT. By F. CHAMPNEYS.

FOR convenience sake, the heart, both of the frog and rabbit, is imagined with its long axis vertically placed, not in the oblique position which it has *in situ;* and the relations of the various parts are given accordingly.

SEPTUM ATRIORUM OF FROG.

In order to learn thoroughly the structure of the septum, it was necessary to have recourse to various reagents. Of these three were found to furnish together all the necessary data, viz. gold chloride solution, silver nitrate solution, and alcohol. In all cases injection by means of a barometer-tube was found most effective. Some experience was necessary to accomplish this satisfactorily; the two difficulties being, 1st, to avoid coagulation of blood on the septum, 2nd, to avoid tearing the very delicate auricles by attempting too much preparation, or by trying to tie too many vessels. The heart should be injected while still beating if possible, at any rate as fresh as possible. It should never be removed from the body till after injection, partly to avoid the risk of tearing the auricles, partly to keep the pulmonary veins, which are much too small to be tied, intact, so as to be able to maintain the pressure.

Ligatures were placed on the large veins, and all were secured except the cava inferior, into which a canula was tied. The right aortic root was tied close to its origin, and a ligature was placed round the left aortic root as far distally as possible, but not tied.

Silver solution.—Silver nitrate solution of ¼ per cent. was simply injected for fifteen minutes, the left aortic root being cut beyond the ligature so as to rapidly clear the heart of blood. When the solution had run through for some time this vessel was tied, and the pressure maintained by adding fresh solution. The heart was removed quickly and the septum at once dissected out under water, exposed to the light, and mounted in glycerin. This gave a beautiful picture of the endothelium and muscular fibres.

Gold solution.—The heart was injected in the same manner with water acidulated with acetic acid for about four minutes, the left aortic root being left open; then with ½ per cent. solution of gold chloride for ten minutes, the left aortic root being tied soon after the beginning of the gold injection, but not at first, in order to remove the acidulated water. After ten minutes' injection of gold solution acidulated water was again injected, the left aortic root was cut to allow the gold to be removed, and then tied again. The heart was then removed from the body, and allowed to hang in acidulated water exposed to the light till it had acquired the proper colour, the pressure of acidulated water being maintained. This gave a beautiful picture of the muscular and nervous structures.

Alcohol was simply injected as above, except that the heart was first washed out with ½ per cent. Na Cl solution to clear it of blood, which otherwise would have coagulated on the septum. The Na Cl solution was removed in the way above described, the aortic root not being closed till the entrance of the alcohol was seen through the transparent wall of the auricle, to cause no disturbance by its mixture with the Na Cl solution, the latter being in fact removed. The pressure with alcohol was maintained for some hours, the heart being allowed to hang in a vessel of alcohol, and subsequently prepared under alcohol. This gave a beautiful picture of all the constituents of the septum except the endothelium, but especially of the connective tissue and muscular striæ.

In dissecting out the septum it was always found convenient to cut off the apex of the ventricle and to blow a few air-bubbles into the auricles, especially the left. By this means the somewhat difficult relations of the very delicate septum are made plain. It should be prepared from the left auricle.

General structure.—The septum atriorum of the frog is a complete partition[1], as is that of the embryo of the bird and mammal. With regard to the description of Sindes and Rokitansky, that the septum atriorum in an early embryonal stage is quite complete, and begins then to form a network, one hole of which remains as foramen ovale—the question now is, whether the septum atriorum of the frog corresponds with the early stage, before the network (in which case the network never exists in the frog), or whether the network first exists and then disappears.

There is no trace of a foramen ovale. Sabatier[2], in a work published in the last two or three months, takes the following position. The venous division of the heart, he says (pp. 187—190), is composed of two divisions, the sinus venosus and the auricle proper; these are quite separate in fish, and really so in Batrachia, but in the higher orders they generally become blended. The septum atriorum of Batrachia is the "septum *auriculorum*," and not the "septum *sinus venosi*," and the foramen ovale (foramen Botalli) is found in Batrachia in the sinus venosus. I have not been able, having so lately gained access to his book, to examine the sinus venosus[3].

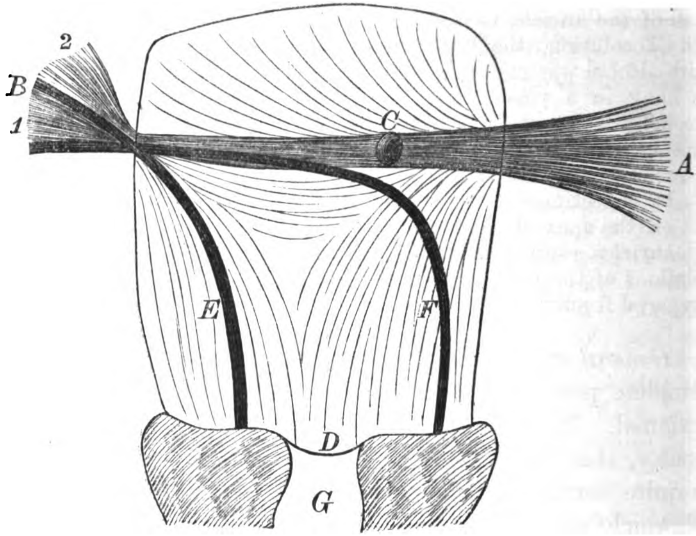
[1] Ueber Defect der Scheidewand der Vorhöfe. Fragment von Carl Rokitansky, *Wiener Med. Jahrbuch*, 1871, p. 113.

[2] *Etudes sur le Cœur dans la Série des Vertébrés.* 1873.

[3] He says (p. 190), "Chez les Batraciens, dont les oreillettes sont uniquement constitués par les auricules, la cloison antérieure (septum auriculorum) forme

Thus the septum atriorum of the frog corresponds only to the very restricted anterior portion of the septum atriorum of

Fig. 1.



Septum atriorum of the Frog (diagrammatic).

A. Anterior muscle.
B. Posterior muscle
 1. Lower horizontal portion seen from the right.
 2. Upper portion radiating vertically seen from the left.
C. Place of insertion of muscle A.
D. Free lower margin of the septum.
E and F. Nerves.
G. Cavity of the ventricle.

entièrement la cloison des oreillettes. C'est naturellement au point de rencontre de ces deux cloisons qu'a du se trouver l'orifice de communication des deux systèmes veineux, c'est à dire le trou de Botal (foramen ovale). Cet orifice se trouve donc, chez le crapaud, la grenouille, la salamandre, et peut-être chez tous les Batraciens, dans la partie antérieure et supérieure de la cavité du sinus, et appartient chez tous au sinus, et non aux auricules." Each of the two divisions has its septum (p. 187). "A chacune des deux parties des oreillettes (sinus et auricule) correspond, chez les Vertébrés qui ont deux oreillettes, une cloison qui lui est propre; l'une antérieure, placée entre les auricules, 'cloison des auricules,' et l'autre postérieure, 'cloison du sinus,' partageant le sinus en deux compartiments plus ou moins inégaux; le sinus des veins caves, et le sinus des veines pulmonaires. La rencontre et la soudure de ces deux demi-cloisons, qui s'avancent l'une vers l'autre, donnent lieu à la séparation complète des deux oreillettes. L'intervalle plus ou moins grand qui existe entre ces deux demi-cloisons, avant l'époque de leur rencontre et de leur soudure, constitue précisément l'ouverture à laquelle chez les Vertébrés supérieures on a donné le nom 'Trou de Botal.'"

Mammals[1], the whole of the rest being represented in the frog by the "septum sinus venosi."

The septum atriorum of the frog hangs with a thin free edge, Fig. 1 D, looking into the ventricular cavity. It shuts off a small room (the left auricle), since it lies much to the left of the middle line. It is very delicate and transparent; possessing only in the boundary line between its upper and middle third any considerable amount of muscular fibres, though these spread from hence over the whole septum. It is formed of a ground substance, of muscular fibres, nerve-fibres, and nerve-cells, and of two layers of endothelium.

The *ground substance* is immensely rich in connective tissue-cells, lying in very beautifully contoured wavy fibres; the whole having an aspect like that of the embryonal connective tissue of Mammalia. Sometimes the connective tissue-cells are granulated, at other times they are oblong, stellate or irregularly shaped. Sometimes there are wavy bundles, and between them fibres arranged like those in a teazed specimen. The connective tissue fills the interstices in the network of muscular fibres.

The *muscular fibres* are most marked in the upper part of the septum. They are derived almost exclusively from two sources. The greater part come originally from a well-marked muscular band, which runs in the horizontal furrow seen on the anterior aspect of the right auricle, and which curving to the left and then backwards, becomes united with the right aspect of the septum in a line at the junction of the upper with the middle third, and at some distance from its anterior edge, Fig. 1, A, C. The triangular space, bounded in front by the anterior wall of the auricle, on the left by the anterior part of the septum, between its anterior edge and the insertion of this

It is difficult to decide whether these two "demi-cloisons" of Sabatier correspond with the "zwei Leisten" of Rokitansky (loc. cit. p. 112), for Sabatier makes no mention of the early embryonal Gitterwerk. The accounts of Rokitansky and Sabatier as to the formation of the foramen ovale obviously differ in this way:—Sabatier considers it as a hole between the septum sinus venosi and septum auriculorum, Rokitansky as one persistent hole, remaining from the innumerable holes in the primitive Gitterwerk.

The "cloison du sinus" or posterior demi-cloison is the "valvule du trou ovale" (p. 189), and all that lies anterior to this in the mammalian heart corresponds to the "cloison des auricules" or anterior demi-cloison, the same probably as the "eigentliches septum atriorum" of Henle. *Gefässlehre*, Fig. 6.

[1] Sabatier, Pl. xv. Fig. 6, 1.

muscular bundle, A, C, and on the right by the muscular bundle itself, is filled with a very delicate membranous network, almost destitute of muscular fibres. Thus the muscular band forms the strong edge of an otherwise very delicate frænum. The second principal source of the muscular fibres of the septum is a posterior band, also joining the septum at the junction of the upper with the middle third, and derived from the posterior wall of the auricle, Fig. 1, B. This, however, joins the posterior edge of the septum at once, and forms no frænum. With it enter the nerves of the septum from behind. The disposition of these bundles is very similar. Some fibres run horizontally across from each to the bundle of the opposite side, some curve upwards, others downwards. The posterior bundle further requires remark, since the fibres, entering in a flat bundle, are folded on themselves in the same way as the fibres of the tendon of the pectoralis major muscle of man. Viewed from the right side, the fibres at first inferior, Fig. 1, B 1, run superficially and quite horizontally and join those of the anterior bundle; those originally next above them cross under (to the left of) them and run forwards and downwards, while those originally most superior lie deepest (most to the left) of all, and have a nearly vertical course, Fig. 1, B 2. This arrangement seems to denote a somewhat complicated development. It will be seen that the line of junction of the upper with the middle third is by far the most muscular part of the septum[1].

The arrangement of the muscular fibres resembles that in the frog's urinary bladder, and in the best gold specimens one would believe that they are truly smooth fibres, since they are very thin, and have oblong nuclei like those of smooth muscular fibres. Sometimes the oblong nucleus represents the

[1] Sabatier, loc. cit., p. 149, says of the septum of Batrachia: "Une particularité digne de remarque de la cloison inter-auriculaire, c'est son indépendance à peu plus complète des faisceaux musculaires qui tapissent les parois. En effet, elle passe au dessus d'eux en y adhérant légèrement, mais sans interrompre leurs parcours et sans influer en rien sur leur direction et leur disposition *de telle sorte que, la cloison enlevée, il ne reste, sur les parois auriculaires, pour ainsi dire, aucun temoin de sa presence.*"

I must differ from the latter part of this statement. The bands which I have described, always thick and well marked, especially the anterior, prevent this description from being correct; but, as I have said, the greatest number of muscular fibres in the septum are derived from these two bands, and apart from them the septum is almost independent of the auricular walls.

thickest part in the fibres. But when we prepare a septum in alcohol, as above described, all the fibres appear striated. It is to be noticed that naturally isolated fibres of the thinnest possible diameter are striated like the fibrillæ in the muscles of a frog killed in an alcohol bath. The network of muscles is in some places so thick as to cover the whole microscopical field, and sometimes they have interstices nearly as extended as the field of vision in Hartnack, Oc. 3, obj. 8, 25 cent. The whole septum of the frog prepared in alcohol offers us one of the most beautiful combinations of the tissues we have mentioned.

Nerve-fibres and -cells.

Through the complicated network of muscular fibres and connective tissue run nervous fibres, the course of which is marked by ganglion-cells. The physiologists recognise these ganglion-cells as the centra motoria of the frog's heart; compare especially the experiments of Stannius. Ludwig, *Müller's Arch.* 1848, p. 140, has described these nerves, as has also Bidder, *Ib.* 1852, p. 163[1].

To the observations of Bidder I have to add that there are to be found *single* medullated nerve-fibres, running a long solitary course, and furnished at intervals with single ganglion-cells, often set on alternate sides, like the buds on the twig of a tree. I have seen one of the two principal nerve-trunks divide

[1] The latter says, "Die beiden gleichstarken Herzäste des Vagus an der Vorhofswand zusammentreffend sich zu einem Knotenoder Plexus vereinigen, aus welchem zwei Fäden von verschiedener Dicke hervorgehen, die divirgirend am vorderen und hinteren Rande der Scheidewand verlaufen. Der vordere ist dünner und länger, der hintere kürzer und stärker, beide aber bilden, sobald sie mit dem Septum an dem Rande der Kammer gelangt sind, jedenfalls eine starke sehr kenntliche Ausschwellung......Auf ihrem Verlauf in der Scheidewand zeigen diese beiden Nerven wiederum reichliche Ganglien-formation, die aber in Einzelnen grosse Verschiedenheiten darbietet, indem die Kugeln entweder vereinzelt ligen und auf der ganzen Strecke ohne erhebliche Unterbrechung sich folgen, oder in grössere und kleinere Gruppen mit dazwischen liegenden gangliosen Partheien gesammelt sind. Immer aber, welches auch vorher das Verhältniss der Kugeln zu den Fasern gewesen sein mag, erscheint an beiden Nerven nahe vor ihrem Uebergang in den Ventrikel ein microscopisches Ganglion von welchem aus dieselben in einer kurzen Strecke ganglienlos zu dem ringförmigen klappenartigen Wulst sich fortsetzen, der den Uebergang in den Ventrikel bezeichnet. Au fdiesem Wege geben beide Scheidewandnervstämme mehrere feine, oft *nur aus zwei Primitivfasern bestehende* Aestchen ab, *die gewöhnlich bei ihrem Abgange am Hauptstamme, nicht aber in weiterem Verlaufe mit Nervenzellen versehen sind.* Diese Aestchen endigen übrigens nicht an dem Septum selbst, sondern setzen sich in die aüssere Wand der Atrien fort."

and then unite again. The longer (anterior) and somewhat thinner nerve-trunk (F) runs at first horizontally along the band of muscular fibres formed by the horizontally running fibres of the two principal muscular bundles, but near the insertion of the anterior bundle (C) it suddenly curves down and runs almost perpendicularly to the ventricle. With the exception of the first part of the course of this nerve, the nerves run generally independently of the muscular bundles. The posterior and somewhat thicker nerve-stem (E) runs almost immediately perpendicularly straight to the ventricle. I must differ from Bidder in the last sentence of his which I have quoted; in no case have I found even one single nervous fibre unconnected with nerve-cells in any part of its course. The best preparations for studying the nerves are certainly those with gold choride, and Bidder used no reagents.

The nerve-cells are in some cases imbedded in the substance of the larger nerves, but often they are laterally placed, though in close apposition to the nerve. They are mostly pyriform and unipolar, with a maximum diameter about three times that of a red blood-corpuscle. Their arrangement and appearance on the larger trunks correspond with those figured by Mayer[1] in "the Sympathetic of the Frog."

Endothelium.—The Endothelium has the usual character of large plates, varying in size and shape in different parts of the septum, and on directly opposite sides. Their shape is irregularly square or long and diamond-shaped, and between these all intermediate forms are found. The contours are tolerably straight or very wavy, and silver shews round dark balls in many places between adjacent cells. Sometimes we find among them pigment-cells. The endothelium covers both sides of the septum.

Blood-vessels.—There are no blood-vessels in the septum of the frog as there are in that of the rabbit. Hyrtl has shewn that the whole of the frog's heart is destitute of blood-vessels.

[1] *Beobachtungen und Reflexionen über den Bau und die Verrichtungen des Sympathischen Nervensystemes.* Fig. 10.

Septum Atriorum of the Rabbit.

Methods of Preparation.

Injection of the heart with various reagents was tried, but furnished no good results. The method at last adopted with some success was to carefully cut out the septum from the heart, if possible still beating, to stretch it out on wax with glass pins, raising it a little from the wax so that the septum was nowhere in contact with the wax, then to plunge the whole into acidulated water for a few minutes, then to remove the superfluous water from beneath the septum by shaking it thoroughly; then either to plunge the whole into ½ per cent. gold chloride solution, or to keep it constantly wet with this solution, by adding the solution continually in small quantities from a drawn-out glass tube. It was exposed to this fluid for a time varying from five to ten minutes, according to the thickness of the septum, then left in acidulated water till it acquired the proper colour. After this it was removed and laid on a glass slide with glycerin and covered with a cover-glass. But only in thin septa did the glycerin make it transparent enough for examination; in grown-up animals it was too thick. The best results were furnished by removing the septum, after it was coloured, from the acidulated water, laying it for some time in alcohol, pressing it gently with a glass rod to help the removal of the water; then a few minutes in oleum caryophyllorum, and mounting it in damar. This shewed the course of the nerves and the position of the cells better than any other method, but the specimens soon became too dark for use.

To learn the course of the muscular fibres the fresh septum in glycerin, stretched out on the edges of a wax-cell mounted on a glass slide, furnished the best results; hyperosmic acid furnished no good results.

For sections the whole septum was treated as above with gold chloride, but it was exposed to the solution for fifteen minutes to penetrate the thicker parts, and only exposed to the light in acidulated water for a short time in order that the surface might not be too dark. It was then imbedded in a mixture of wax and oil and cut with free hand in the usual manner.

Nerves.—There was often a considerable, though microscopic, nerve-trunk, which ran in general vertically downwards across the fossa-ovalis, and was apparently intended for distribution in the septum-ventriculorum, which destination its direction, its considerable size throughout, and the fact that it rarely gave off a single branch, seemed to denote. This nerve was sometimes accompanied by an artery of equal size. Sometimes this nerve ran from the anterior and superior part of the limbus, near the foramen-ovale, curving backwards; oftener it

was found at the posterior part of the fossa-ovalis, sometimes even in the muscular bundle forming the posterior boundary of that fossa. In two cases I found two or three very minute ganglion-cells imbedded in this nerve as it crossed the fossa-ovalis. In addition to this there were sometimes one or two minute nerves having a similar direction, but not extending far, and ending in the septum.

In the limbus and muscular boundaries of the fossa-ovalis there were sometimes many nerves to be seen, having in general the same direction as the above, and in certain cases a plexus, which was almost invariably in connection with the ganglion-mass, to be shortly described, and therefore in the posterior and superior part of the limbus, above and behind the fossa-ovalis. In one case where the ganglion was situated lower down than usual, viz. behind and below the fossa-ovalis, the plexus was there also, but in one case where no ganglion could be found in the usual place, the plexus was still there[1].

Nerve-cells.—The nerve-cells in the septum atriorum of the rabbit are of very different sizes and kinds. I distinguish three varieties. (1) The first sort are very small, having a maximum diameter generally half that of a red blood-corpuscle. These I have found embedded deeply in the substance of the larger nerves, and are the only nerve-cells which I have found any distance removed from the muscular parts; I have for instance found two or three of these as above described, embedded in a large nerve as it crossed the fossa-ovalis. They form no ganglion-mass. (2) The second have a maximum diameter about twice that of a red blood-corpuscle, and are by far the most numerous. These constitute the principal ganglion masses. (3) The third are rare. They have a maximum diameter about five times as great as that of a red blood-corpuscle, and are found not thrust into the substance of nerves or forming considerable ganglion-masses, but I have found them connected with a single medullated fibre, as in the frog. These are pyriform and unipolar, and resemble those found in the septum of the frog, except that they are larger[2].

[1] Scarpa in his work on the nerves of the heart mentions (*Tabulæ Neurologicæ*, 1794, p. 2) nerves in the auricle, and figures them (*Tab.* VII. Fig. 2), but only on the exterior.

[2] Lee in his treatise on the nerves and ganglia of the heart (*Phil. Trans. of*

With regard to the position of the ganglion-cells I can affirm that the first variety (1) are the only nerve-cells which I have found removed from muscular tissue. I have found these embedded in the substance of the principal nerve which I have described as crossing the septum, and also in other large nerves. I have never found more than six of these together, and these not in close apposition.

The second variety (2) form the principal ganglion masses. With regard to the position of these, I can affirm, that, in spite of some irregularities, I have found them in by far the greatest number of instances forming a well-marked mass, occupying a constant position, viz. in the muscular boundary of the fossa-ovalis at its posterior and superior part, between the openings of the right vena superior and cava inferior (Fig. 2 c). Sections shew that it lies not embedded in the muscular substance, but on the right side of the septum and superficial to the muscular fibres. It is often in connection with a considerable plexus of fine nerves, as above stated. The number of cells varied with the age of the animal, as will be mentioned.

As irregularities I have found this mass lower down than usual, viz. behind and below the fossa-ovalis, and in one solitary instance, where the place usually occupied by this ganglion was devoid of cells and of the usual plexus of nerve-fibres, I found a large mass of several hundred of these cells below and a little behind the foramen-ovale, but this mass lay on the *left* side of the septum. This was in an adult rabbit. I have also in one large rather old individual found nerve-cells in nearly the whole of the limbus, but as this was a lamellated preparation, I cannot give their exact position; I never subse-

Royal Soc. of London, 1849, p. 47), treats, neither in the text nor figures, of the nerves or ganglia in the auricle, still less of those in the septum. His descriptions are quite general, as the following extract shews) (p. 47): "It can be clearly demonstrated that every artery distributed throughout the walls of the uterus and heart, and every muscular fasciculus of these organs, is supplied with nerves upon which ganglia are formed." If this is so the innervation of the mammal's heart must be essentially different from that of the Frog, according to Bidder's investigations, for only in the neighbourhood of the septum auriculorum could he find ganglia or nerves.

Neither Scarpa nor Bidder mention the nerves or ganglia of the septum auriculorum in man or in any of the mammals which they have examined.

Krause, in his work on the anatomy of the rabbit, also says no word on this subject.

quently found them so dispersed. These were, however, exceptions to the vast majority of instances.

Fig. 2.

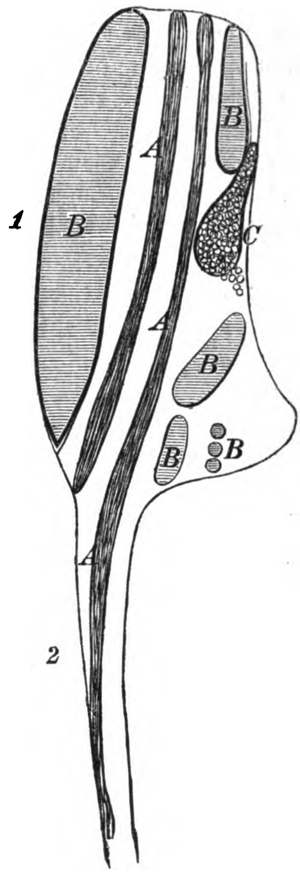


Diagram of a longitudinal section from the upper half of the septum atriorum of the rabbit.

A. Muscles cut longitudinally.
B. Muscles cut transversely.
C. Ganglion.

Differences in relation to age.—Two great differences may be noticed between the septum of young and adult rabbits; age increases the richness, first of muscular fibres, then of nerves

and nerve-cells. In the young rabbit the floor of the fossa-ovalis is entirely composed of connective tissue, and has no muscular fibres. Sabatier makes the same remark (p. 189) of Birds as well as of Mammals, and he says that the addition of muscular fibres is most marked in the larger animals. I have not had time to study the genesis of the muscular fibres among the connective tissue of the septum, but as this takes place long after birth, and as the floor of the fossa-ovalis is very thin in young animals, there could hardly be found a better place for studying this most interesting process. I may state, however, that I have seen no signs of the process being one of gradual encroachment from the periphery, and imagine the genesis takes place immediately *in situ* and independently of the surrounding muscular masses, though these also grow. There is often a marked growth of muscular fibres, constituting the vertical band which sometimes divides the fossa-ovalis into two parts. This absence of all muscular fibres in the young animal in the floor of the fossa-ovalis is one of which any one may convince himself, the thinness of the septum favouring the examination. The muscular and nervous constituents seem to grow in dependence on one another, for both of these are far less plentiful in the young than in the old individual; and, moreover, with the single exception of the nerve which I have described, which seems destined for the septum ventriculorum, and the two or three nerve-cells which I have twice found embedded in it, nerve-fibres were, however small, hardly ever, nerve-cells never found, except in the close neighbourhood of muscular fibres.

Anomalies.—The following anomalies have incidentally struck me in my work:—

(1) In one young rabbit I found a small free band of fibres running from the anterior border of the septum, i. e. from the anterior part of the limbus, across the fossa ovalis, and joining the septum again at its posterior border. Henle (p. 8), says, "Zuweilen spannt sich ein Gitterwerk feiner Fäden über die Oeffnung:" this of course refers to man.

(2) I found, in both a young and an old rabbit, the foramen ovale entirely shut, but the "valvula foraminis ovalis" with a perfectly free anterior border. This apparently contradicts the usual account which says that the foramen ovale is closed by the forward growth of the

left of the two falces ("halbmondförmige Saüme," Henle), or at any rate shews that this is not always the method. Indeed, from the observations of Rokitansky the septum is developed as a perfect partition which *afterwards*, becomes perforated with innumerable holes forming "Gitterwerk," and the falces are a later growth[1].

(3) I have found two openings in the septum, which was very thin, in an adult rabbit. Neither of these occupied the position of the normal foramen ovale, but were more posteriorly placed; they were not surrounded by any rim of fibres, nor provided with any valve, but were simply formed by the divergence of the scanty fibres, in this case formed simply from the left falx or valvula foraminis ovalis. These were no doubt persistent holes in the original "Gitterwerk" of Rokitansky[2].

In conclusion I may add, that in the course of my work I have many times coloured with gold and carefully examined the semilunar and atrio-ventricular valves for nerve-fibres and cells, as well as the papillæ from both ventricles, but with the exception of one doubtful case, in which a mitral valve seemed to have a few cells which looked like nerve-cells close to its attached border, there was never any trace of either to be seen. The papillæ, however, must sometimes, or in some animals, possess nerve-cells, for Stricker has once seen the freshly excised papilla from one of the ventricles of a dog, independently and rhythmically contract.

[1] He says (*loc. cit.* p. 113), "Indem diese Courtine sich weiter herablässt, erreicht ihr unterer freier Rand endlich die oben beschriebene quere auriculo-ventricular-Spalte, überbrückt dieselbe, sich mit ihr rechtwinklig kreuzend, und verschmilzt mit deren Lippen, welche unter einem auch mit einander verwachsen. Es ist der Vorhofsack hiemit in zwei Raüme gesondert," and (p. 114), "Der das ursprünglich edurchbrochene haütige Septum—das Gitter—umfassende dickere Rahmen wächst ringsum."

[2] He says (p. 115, 3), "Dabei kommen seine Löcher durch Wachsthum der sie umfassenden Balken in grosser Anzahl zum Verschlusse, während andere bleiben und sich vergrössern."

PRELIMINARY NOTE ON AN EPITHELIAL ARRANGEMENT IN FRONT OF THE RETINA AND ON THE EXTERNAL SURFACE OF THE CAPSULE OF THE LENS.—By J. C. EWART, *Student of Medicine, University of Edinburgh.*

MAX SCHULTZE states that the membrana limitans interna of the retina immediately invests the vitreous humour to which it is often intimately connected, that it is partly formed by the supporting fibres which traverse the layer of the optic nerve fibres, these fibres being arranged serially and prolonged into conical flattened enlargements, or after undergoing division, like roots of a tree, are continuous with several such terminal dilatations that ultimately coalesce to form the smooth membrana limitans interna.

Kölliker described this membrane as intimately connected with the retina, but differing from it in consistence; hence he ranked it amongst the vitreous membranes. Henle also considered it an independent membrane, to the outer surface of which the radial-supporting fibres of the retina, with their expanded extremities, are applied, calling it the limitans hyaloidea, to shew its identity with the special membrane of the vitreous humour; but Max Schultze, though fully recognising the importance of the separability of the m. limitans, and the difference in resistance between it and the spongy subjacent layer, considers it identical in structure with the supporting spongy connective tissue. That the internal limiting membrane is not composed of spongy connective tissue, or that, besides the limiting membrane already described, there is another epithelial membrane lying on the anterior surface of the retina, may be demonstrated by the following method.

Take a fresh ox's eye, divide it transversely into an anterior and posterior part. From the posterior half evacuate the vitreous humour, and pour in some half per cent. solution of nitrate of silver; in ten minutes wash this out with distilled water, and expose for a short time in a good light. On examining the surface of a retina, thus prepared, next the vitreous humour

with a No. VII. objective and No. 4 eyepiece (Hartnack) a beautiful mosaic of epithelial cells is seen. The cells, varying from a faint yellow to a dark brown, are multiform, fitting closely into each other, constituting a continuous layer, which can be traced over the whole retina. Some of the cells (with No. VII. and 4) are nearly half an inch long, and from one to two lines in breadth. Filling up the small spaces between the larger cells are many small ones, sometimes only half the size of a blood-corpuscle, and more or less hexagonal, thus forming a centre, from which larger cells are often seen radiating. The edges of the cells are slightly irregular but not serrated, the surface next the vitreous humour is smooth, and if the eye has been quite fresh, even after the addition of nitrate of silver, well-marked nuclei may be seen, either as small round spots, or as irregular crescent-like bodies. When treated with hæmatoxylin these nuclei are brought well out, and have often the same irregular appearance. If after staining with silver the preparation is left some time in strong acetic acid the whole is rendered more transparent, and the nuclei of the cells, both of the epithelial layer and those of the vascular sheath, become visible. The retina being very vascular, at almost any part, on altering this focus, you see one or more of the vascular sheaths discovered by His. The sheaths are easily distinguished from the fine epithelium on the surface by the different arrangement of the cells, which are generally larger and often run at right angles to those of the epithelial layer. The epithelium cells of the epithelial layer over the smaller vessels and capillaries are long and narrow, bridging over the space like so many small arches. Over the large vessels sometimes there are two, or three, lengths of elongated narrow cells; sometimes they have their ordinary form, but with many small cells between the large ones, as is well seen where the vessel has ruptured, leaving the epithelium above entire. The sheaths of the small vessels and capillaries are chiefly composed of elongated cells similar to, but better marked than, those figured by Dr Thin in his paper on the "Lymphatic System of the Cornea[1]." In each cell an oval nucleus can be seen when hæmatoxylin and acetic acid are added to a silver preparation. Besides these long

[1] *Lancet*, Feb. 14, 1874.

pointed cells in the larger vessels numerous irregular cells run across the tube, seemingly on a deeper plane; these are best seen when the silver has entered the lumen of the tube through its probably staining the cells of the intima. The sheaths envelope all the vessels, and not only the small and medium-sized ones as formerly described.

The epithelial layer above described extends over all the surface of the retina, but I have not been able to trace it beyond the ora serrata on to the ciliary processes. Immediately under it the vessels and connective tissue can be seen, and between the bundles of connective tissue spaces are left which in the fresh state are filled with nerve-fibres.

Lying on this layer of epithelium is the vitreous humour. The vitreous humour is described as having no distinct capsule, and the posterior three-fourths of it can easily be removed, but at and near the ora serrata some of the fibres of the outer layers of the vitreous humour dip into the substance of the retina through narrow spaces left in the epithelial lining, and thus the two firmly adhere. The vitreous humour is also adherent by some of its external and connective tissue-like fibres to the ciliary processes, but has no attachment to the posterior surface of the lens.

In the sheep the epithelial layer is easily demonstrated, it only differs from that of the ox in that the majority of the epithelial cells are smaller. At some parts, especially over the vessels, there are groups of large cells; these however are not elongated, as in the ox, but are generally circular, and, when treated with acetic acid, present a distinct round nucleus with a nucleolus. The lymphatic sheaths are composed of long pointed cells with oval nuclei; at a deeper plane numerous nuclei run across the tube, corresponding to the cells already described running across the vessel. On the surface of the retina next the choroid there is exactly the same appearance, as that figured by Professor Henle in the epithelial layer of the olfactory region of the horse (Fig. 642, p. 834, vol. II.).

In the cat the epithelial cells are nearly all of the same size, they are smaller but more regular than those of the ox. The sheaths round the capillaries are especially well marked, and as in the sheep, numerous nuclei are seen on some of the sheaths.

In the common fowl the epithelial cells are exceedingly small —few being as large as a human blood-corpuscle. The greater number are nearly circular, others are four, five, or six-sided, all uniting to form what appears with a No. VII. a very fine network. Under this thin layer fine parallel bundles of connective tissue are seen, which have a striated appearance like a muscular fibrilla. This striated appearance is due to clear spaces which, in the fresh state, are filled with fine nerve-fibres. On the surface next the choroid the inner segments of the cones —each ending in a bright red or yellow lenticular body—are seen passing through the round spaces of the external limiting membrane.

LENS.

On taking the anterior half of an ox's eye, removing the vitreous humour from the posterior surface of the lens, and treating with silver, as before, a layer of epithelium is found on the posterior surface of the capsule.

Babuchin says that several authors have found epithelial cells on the posterior surface of the capsule of the lens, which probably resulted from the circumstance, that the inner surface of the anterior capsule has been described as covered with epithelial cells. He thinks it would be more natural to say that the epithelium which forms the anterior layer, and the direct continuation of the posterior, as well as this last itself, is covered by the capsule. He thinks that the posterior extremities of the fibres of the lens, which directly abut against the capsule, or the spherical bodies which arise from the breaking up of these ends, have been taken for the epithelial cells.

Whatever kind of epithelial cells may have been described, those seen by treating the capsule as above could not be any of the structures Professor Babuchin mentions. In the ox the cells are large, sometimes measuring half an inch each way with No. VII. Hartnack: they are polygonal: the nucleus is of a bright yellow colour, and sometimes under that objective seems as large as a small pea. The edges are irregular, and processes from one cell dovetail into those immediately in contact with it. The surface is smooth, and under the cells the outer fibres of the capsule of the lens are seen. At some parts spaces are left

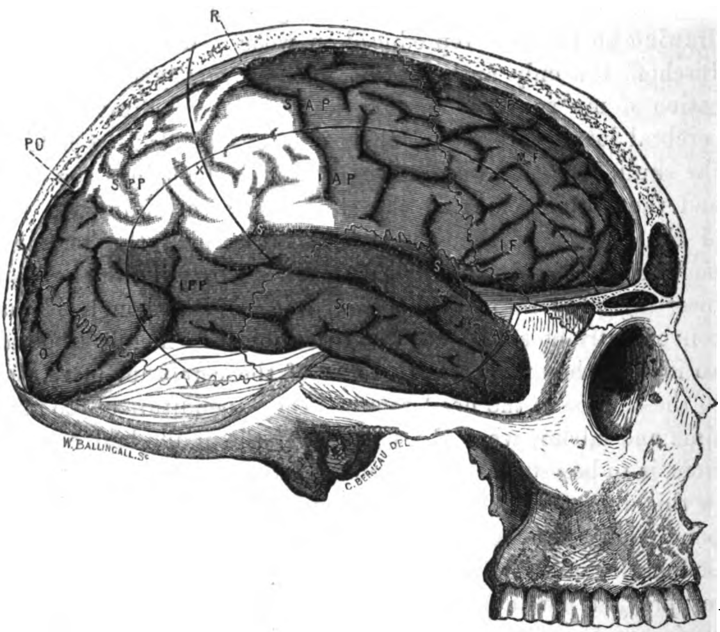
between the fibres which communicate with each other, thus corresponding to the spaces in the cornea which contain the branching cells. At a lower level the characteristic serrated fibres of the lens come into view.

In the domestic fowl all the cells but a few round the margin are hexagonal—the sides of each being twice as long as the ends—there is a nucleus about half the size of the one in the ox, and, as in the ox, there is a nucleolus. These cells are arranged in parallel rows, and under each row is a large bundle of connective tissue; between each bundle is a small lymphatic space, similar to those between the bundles of connective tissue of tendons; these spaces as well as the bundles of fibres are covered over by the epithelial cells—each row of cells fitting exactly into those on each side of it.

Near the margin of the capsule the cells are nearly square, and not arranged in parallel rows as in the centre. Immediately behind this epithelial layer is the vitreous humour, which is seen to be in contact with epithelial cells nearly all round, but only adherent at the ora serrata and the ciliary processes.

In a puppy-dog four days old well marked lymphatic sheaths are seen, similar to those of the retina, immediately under the posterior epithelium. The cells of the posterior epithelium are, except at the margins, hexagonal, and the edges are distinctly serrated. At the margin they are often irregular in shape, and sometimes not serrated. On the anterior surface of the capsule of the lens next the aqueous humour the epithelial layer of cells is similar to those on the posterior surface of the cornea.

of the skull. Taking the sutures as the guides to the primary divisions, I spoke of an occipital, or post-lambdoidal; a frontal, or præ-coronal; a parietal, subdivided into an antero-parietal or post-coronal, and a postero-parietal or præ-lambdoidal, by a line drawn vertically through the parietal eminence from the squamous to the sagittal suture; and a squamoso-sphenoid. The frontal region was then sub-divided into a supero-frontal, a mid-frontal and an infero-frontal area: the antero-parietal into a supero- and an infero-antero-parietal: the postero-parietal into a supero- and an infero-postero-parietal: the squamoso-sphenoid into a squamoso-temporal and an ali-sphenoid region. The occipital area was undivided.



R. Fissure of Rolando, which separates the frontal from the parietal lobe.

P. O. Parieto-occipital fissure between the parietal and occipital lobes.

S. S. Fissure of Sylvius which separates the temporo-sphenoidal from the frontal and parietal lobes.

SF. MF. IF. The supero, mid, and infero-frontal sub-divisions of the frontal area of the skull: the letters are placed on the superior, middle and inferior frontal convolutions.

SAP the supero-antero-parietal area of the skull: S is placed on the ascending parietal convolution, AP on the ascending frontal convolution.

IAP the infero-antero-parietal area of the skull: I is placed on the ascending parietal, AP on the ascending frontal convolution.

SPP the supero-postero-parietal area of the skull, the letters are placed on the angular convolution.

IPP the infero-postero-parietal area of the skull, the letters are placed on the mid-temporo-sphenoidal convolution.

X the convolution of the parietal eminence, or supra-marginal gyrus.

O the occipital area of the skull, the letter is placed on the mid-occipital convolution.

Sq the squamoso-temporal region of the skull, the letters are placed on the mid-temporo-sphenoidal convolution.

AS the ali-sphenoid region of the skull, the letters are placed on the tip of the supero-temporo-sphenoidal convolution.

ON THE PLACENTATION OF THE SLOTHS.

By Professor Turner.

In this *Journal*, June, 1873, a short abstract was given of a memoir communicated to the Royal Society of Edinburgh on the Placentation of the Sloths. This memoir has now been printed *in extenso* and illustrated with four quarto plates, in Vol. xxvii. of the *Transactions* of that Society. The memoir opens with a short historical introduction, in which the meagre observations of Rudolphi and G. C. Carus on the placenta in *Bradypus* are referred to, and the comments thereon by Von Baer, H. Milne-Edwards, Owen, Huxley, and Rolleston. A detailed description of the gravid uterus of *Cholopus Hoffmanni* and of the naked-eye and microscopic characters of its placenta is then given. The characters of the fœtus and of the remarkable envelope named by Welcker the Epitrichium are then described. The memoir concludes with a chapter entitled "General Observations on the Placentation of the Edentata;" and as in this chapter the results arrived at by the dissection are summarised, it is reproduced here—

General Observations on the Placentation of the Edentata.

From the description given of the form and structure of the placenta in this specimen of *Cholopus*, and from the accurate interpretation which I am able to offer of Carus's figure of the placenta in *Bradypus*, it is evident that in the Sloths the placenta is not cotyledonary, *i.e.* if we use the term cotyledon, in the sense in which it is usually employed by zoologists, to express a particular form of non-deciduate placenta, subdivided into distinct and scattered masses as in the Ruminants. It can only be called cotyledonary, if the term be employed, as is sometimes done in speaking of the deciduate human placenta, as equivalent to lobes. To avoid confusion in the use of terms, it may be well to speak of the sloth's placenta as a dome-like, multilobate, aggregate placenta, the lobes of which are discoidal. It is a deciduate placenta in the fullest sense of the word; for not only is a decidua reflexa shed along with the fœtal membranes, but if the plane at which I separated the placenta from the uterus be, as I think there can be no doubt it is, the natural plane of separation of these parts during parturition, then the fœtal membranes carry away with them the deciduous serotina, the curling arteries, the utero-placental veins, and the intra-placental maternal vessels. I have been able, therefore, to put on the basis of an actual demonstration the deciduate structure of the placenta in the Sloth, which Rolleston, Owen, and Milne-Edwards, from the study of Carus's drawing, had regarded as not improbable.

The character of the placentation in the Sloths having now been determined, it will be interesting in the next place to compare it

with what has been recorded respecting the placentation of the other mammals included in the order Edentata. Unfortunately, however, we are not provided with such detailed information relative to the placental characters of these animals as to enable one to make so complete and exact a comparison as could be desired. Any remarks, therefore, which may be based on this comparison must be regarded as provisional merely, and to be subject to revision when more precise knowledge is obtained.

Of the placenta of the Armadillos nothing further has apparently been recorded than is contained in the brief statement made by Prof. Owen[1], that it is a single, thin, oblong disc, with which the maternal deciduous substance is interblended.

Our information as to the placenta of *Orycteropus* is also equally brief, and is limited to the observation recorded by Prof. Huxley in his "Introduction to Classification[2]," based on the examination of a specimen in the stores of the Royal College of Surgeons, London, that in this genus the placenta is discoidal and deciduate.

Of the Hairy Anteaters, A. F. J. C. Mayer states[3] that in *Myrmecophaga* (*Cyclothurus*) *didactyla* a single orificium uteri exists, and in the gravid state the uterus and vagina are blended into a common cavity, and form a round membranous sac. The fœtus in the specimen he examined was well developed and strongly haired, with the head presenting. The placenta was a thick roundish cake (*Kuchen*), and lay to the right in a special pouch of the uterus. The chorion and amnion were distinct, but the erythrois and allantois could not be distinguished in the torn membranes. Prof. Welcker, in his Memoir[4], incidentally mentions that in *M. didactyla* the amnion and chorion circumscribe in the usual way the border of a fungiform (*pilzförmig*) placenta. The elder Milne-Edwards[5] states that he has found in *M. didactyla* a discoid placenta, but composed at its borders of small branched tufts: it did not appear to be united to the uterus by a decidua. His son, M. Alphonse Milne-Edwards, described only last year[6] a placenta of *Tamandua tetradactyla*, which had been hardened in spirit for some years before it came into his possession. It occupied the larger part of the surface of the ovum, and was not composed of simple villosities like the placenta of the Pachydermata, but of vascular very compact vegetations. Its central part was thick and spongy; its borders well defined, with a smooth chorion beyond them. The vascular vegetations, he says, do not apparently resemble the reticulated folds and alveoli seen by Dr Sharpey in *Manis*. Some débris of uterine tissue indicated the presence of a decidua, but he could not speak positively on the subject. The placenta is unilobed, dome-like in the mode in which it is set on the chorion, and is named by Milne-Edwards *placenta discoïdal envahissant*. The surface of

1 *Anatomy of Vertebrates*, III. p. 731. 1868.
2 P. 104. London, 1869.
3 *Analecten für Vergleich. Anatomie*. Zweite Sammlung, p. 54. Bonn, 1839.
4 *Abhand. der Natur. Gesell. zu Halle*, Bd. IX. 1864.
5 *Leçons sur la Physiologie*, IX. part 2, p. 563, note. Paris, 1870.
6 *Annales des Sciences Naturelles*, XV. 1872.

the placenta next the embryo did not possess the projections seen by Carus in *Bradypus*, or by myself in *Cholopus*, but was smooth. No trace of an allantois was found.

From the above descriptions it is clear that in the hairy ant-eaters the placenta is not diffused over the whole surface of the ovum, but is localised in a particular area. From Milne-Edwards' figure the proportion of chorion occupied by the placenta is about equal to what I have seen in *Cholopus*, but the organ is not, as in the latter animal and in *Bradypus*, subdivided into lobes. From the expressions *Kuchen* employed by Mayer, *pilz-förmig* by Welcker, discoid and spongy by Milne-Edwards, *père et fils*, it is also clear that the organ possessed considerable thickness; and although the younger Milne-Edwards could not, from the condition of his specimen, speak positively of the presence of a decidua, the thick spongy character of the organ with the compact arrangement of the villi points rather to a deciduate than a non-deciduate structure.

In the Tardigrada, the Dasypodidæ, and the Orycteropodidæ, we have evidence then that the placenta is deciduate, and composed of one or more disc-shaped lobes. In the Myrmecophagidæ the evidence is not so complete, though I think it inclines in favour of the deciduate nature of the placenta.

But when we turn to the Scaly Anteaters we find a placenta of a very different character. Some years ago Dr Sharpey examined the gravid uterus of a *Manis*, which, from the size of the fœtus, was presumably near the full time. He observed several most important features in the arrangement and structure of the placenta, which he communicated to Professor Huxley, who incorporated them in his "Elements of Comparative Anatomy[1]." "The surface of the chorion is covered with fine reticulating ridges, interrupted here and there by round bald spots, giving it an alveolar aspect, something like the inside of the human gall-bladder, but finer. The inner surface of the uterus exhibits fine low ridges or villi not reticulating quite so much. The chorion presents a band free from villi, running longitudinally along its concavity, and there is a corresponding bald space on the surface of the uterus. The ridges of the chorion start from the margins of the bald stripe, and run round the ovum. The umbilical vesicle is fusiform."

In a letter to me, in reply to a request for further information about this specimen, Dr Sharpey states that it had been in spirit before coming into his possession, and that the substance of the uterus and the tissues of the embryo were brown and fragile. An injection, both of the uterus and membranes, was attempted, but, from the condition of the parts, was unsuccessful. The elevations on the chorion corresponded to the finely-corrugated inner surface of the uterus. "I turned off the chorion like an everted stocking, and got the arrangement of the allantois, fusiform umbilical vesicle, omphalo-mesenteric and umbilical vessels. The ramifications of the umbilical vessels extended generally over the inner surface of the chorion, and

[1] P. 112. London, 1864.

were lifted off with it from the receptacular part of the allantois, which was very extensive, and passed into the diverticula of the chorion. The uterine glands were abundant and easily seen, but I could never distinctly trace their orifices; it seemed to me as if the ducts opened not abruptly, but gradually and funnel-like among the placental rugæ. I think you found this condition in the whale."

I have not only to express my thanks to Dr Sharpey for this additional information, but to state that with great liberality he has allowed me to examine the original drawings of the chorion and uterine glands, and the specimen itself as it had been dissected by him, and to supplement his description by the following particulars. Although the preparation had now been for many years in spirit of wine, yet I had no difficulty in recognising the diffused arrangement of the fine villi of the chorion and the elongated band free from villi, such as Dr Sharpey had described. In its general aspect the villous surface resembled the appearance which I had seen and described in *Balænoptera* and *Orca*[1], though the ridges, folds, and villi of the chorion were finer than those seen in the Cetacea. The condition and age of the specimen were such as to render it impossible to make a satisfactory microscopic examination of the structure of the villi. The two extremities of the elongated chorion were unequal in capacity, and the non-villous band extended in closer proximity to the more dilated than to the narrower pole of the chorion. In the more dilated end part of the fœtus had, in all likelihood, been lodged; and it is probable that the poles of the chorion had been contained in two pouch-like recesses about the size of walnuts, one situated at each lateral extremity of the transversely elongated uterus. Each communicated freely with the general cavity of the uterus by an orifice somewhat less in diameter than that of the pouch itself, and was lined by a prolongation of the uterine mucosa. Into the outer end of each pouch the very fine orifice of the Fallopian tube opened. I examined carefully the poles of the chorion to ascertain if a spot bare of villi, similar to what I had seen in *Orca* and in the mare, existed. At the narrower end the chorion was torn, so that the examination was not satisfactory, but the more dilated pole was entire, and in it no bare non-villous spot was recognised, so that if, as I suppose, the chorion enters the pouch-like recesses of the uterus, the villi investing it would have a relation to the mucosa lining each pouch as the villi covering the body of the chorion have to the mucosa lining the general cavity of the uterus. I saw no stellate bare spot on the chorion corresponding to the orificium uteri similar to what I have described in *Orca* and in the mare. Branched, cylindriform utricular glands, as figured in one of Dr Sharpey's drawings, closely resembling those I have figured in *Orca*, and containing plenty of epithelial cells, the form of which was not very distinct, though apparently columnar, could be seen without difficulty in the uterine mucous membrane, but I could not precisely ascertain

[1] *As described and figured in my memoirs in the Transactions of this Society*, Vol. XXVI. pp. 207 and 467, 1871.

the mode in which they opened on the surface of the mucosa, which was thickly studded with minute pits or fossæ; it is probable, however, that they opened obliquely into the bottom of these pits. The free surfaces, both of the chorion and uterine mucosa, presented, without doubt, the appearance which one recognises as characteristic of a diffused non-deciduate placenta.

The placenta, therefore, in *Manis*, differs in several most important particulars, both in arrangement and structure, from what I have described in *Cholopus*. Not only is it diffused and non-deciduate, but both the allantois and umbilical vesicle remain as distinct sacs, and the utricular glands persist throughout the gravid uterine mucosa.

The demonstration, therefore, of the diffused, non-deciduate character of the placenta in *Manis* by Dr Sharpey, and of the multilobate, discoid, deciduate placenta in *Cholopus* by myself, will render it necessary for the systematic zoologist to reconsider either the value of the placenta as the basis of a system of classification, or the propriety of retaining the Sloths and the Scaly Anteaters in the same order. If the characters of the placenta are to be regarded as of more importance in classification than those furnished by any other organ or combination of organs, then it is clear that the non-deciduate *Manis* can no longer be placed in the same order as the deciduate Tardigrade. But if the characters of the other organic systems in the animals belonging to the order Edentata, as at present accepted, exhibit a series of affinities, which to the mind of the zoologist may seem to outweigh the differences in placental structure, not only between *Manis* on the one hand, and *Cholopus*, *Dasypus*, and *Orycteropus* on the other, but, if my inference as to the deciduate nature of the placenta in *Myrmecophaga* be correct, between *Manis* and *Myrmecophaga* also—affinities which would render it advisable that they should be retained in the same order,—then the placental system of classification is obviously not universally applicable, and will have to be abandoned. It would be out of place in this communication to enter into the consideration of the anatomy of the other organic systems in *Cholopus*, and to discuss how far its various organs resemble, or differ from, those of the genera with which it is usually associated; but I hope, from the materials in my possession, to supplement this memoir, and draw up, in the course of the next few months, a detailed account of the structure of this animal, and to compare it, as far as the materials at my disposal will allow, with the other animals usually grouped with it in the order Edentata.

Before bringing this memoir to a conclusion, it may not be without interest briefly to compare the placentation of the Sloths with that of the other orders of Deciduate mammals.

It may help to make this comparison more complete if I introduce here some observations which I have recently made on the structure of the unimpregnated uterus of the Sloth. Through the kind permission of Dr Sclater I received from A. H. Garrod, Esq., Prosector to the Zoological Society, the fresh carcase of a female Hoffmann's Sloth, which had died in the Gardens early in the month of November.

An injection of coloured gelatine was passed into the arterial system from the abdominal aorta, and the uterus was then removed from the abdomen. The uterus was $3\frac{1}{4}$ inches long by $\frac{8}{10}$ths inch broad. The Fallopian tubes were slender, the ovaries the size of peas, and lodged in peritoneal pouches. The cavity of the uterus was remarkably large for a non-gravid organ, and through its somewhat constricted orifice which opened into the vestibule, the finger could be introduced into the cavity, which shewed no subdivision into cervix and body.

The mucous membrane both on the anterior and posterior walls was elevated into longitudinal ridges such as I have already described in the pregnant uterus, but these ridges terminated about $\frac{3}{4}$ths inch from the orificium uteri, leaving a smooth surface of mucosa. The mucous membrane was very vascular; the small arteries both in it and in the submucous coat presented a corkscrew-like twist, the coils of which were close together, so that there can be no doubt that the curling arteries of the placenta pre-exist in the mucosa, and merely grow larger during pregnancy contemporaneously with the development of the placenta. The veins were larger than the arteries, and serpentine in their course. The surface of the mucous membrane of the fundus uteri, and both horizontal and vertical sections through its thickness, were examined with reference to the presence of tubular utricular glands. Distinct evidence of their existence was obtained, though they were much more difficult to see and much less numerous than in the pig, mare, cetacean or *Manis;* they were short tubes, and did not appear to give off more than one or two branches, which terminated in rounded closed ends. They did not lie perpendicular to the plane of the surface, for in vertical sections they were irregularly divided, and they were arranged in groups so that they were more numerous in some than in other portions of the fundus. Their orifices on the free surface of the mucosa, which were recognised with some difficulty, were nearly circular in form, and a somewhat elongated epithelium projected from the wall towards the centre, leaving, however, a small lumen; the polygonal ends of the epithelial cells were seen through the walls of the glands as they lay horizontally in the mucosa. Capillary loops surrounded the glands in the deeper part of the mucosa. In the smooth part of the mucosa near the orificium, although a careful search was made, no glands were seen, and the arteries did not possess the corkscrew-like twist, so that from these structural differences this part of the mucosa did not present the same characters as that of the fundus uteri. The connective tissue of the mucous membrane contained numerous well-marked corpuscles, and its surface was covered by a layer of cells, only the ovoid nuclei of which could be defined with precision.

The placenta of the Carnivora is at once distinguished from that of the Sloth by several striking characters. By its zonary form; by the presence not only in the unimpregnated uterus, but in the non-placental area of the gravid uterine mucosa, and in the maternal part of the fully developed placenta, of utricular glands; by the intra-pla-

cental maternal vessels retaining the form of a capillary net-work[1]; by the brevity of the umbilical cord and by the persistence both of the umbilical vesicle and allantois.

In the Insectivora, Rodents, and Cheiroptera again, the placenta, though with some slight modifications in its shape, forms a single "discoid" organ, which in some cases at least shews a subdivision into lobes; the allantois and umbilical vesicle, the latter of which is of large size in many genera, persist as distinct sacs throughout intra-uterine life, and no evidence has been advanced that the intra-placental maternal vessels are dilated into sinuses. Reichert has shewn[2] that in these orders a decidua reflexa, more or less complete, exists. The condition of the mucous membrane, as regards the utricular glands, exhibits some variations in different genera. Leydig has observed them in the mole[3]; Ercolani in the hedgehog[4]; Reichert in the guinea-pig[5]; Ercolani in *Mus musculus*[6]; in which animal he says they are few in number, simple, and slightly sinuous. In the rabbit there is some difference of opinion as to the nature of the deep depressions which exist in its uterine mucosa, for though Reichert compares the windings and folds of that membrane to the appearance exhibited by the convolutions of the brain, and speaks (Müller's *Archiv*, 1848, p. 80) of the involutions of the mucous membrane as short and wide, yet he evidently regarded them as essentially the same as the tubular utricular glands in the pig, guinea-pig, bitch, &c. Ercolani again states that the rabbit, instead of possessing utricular glands, has numerous very short glandular follicles, which are only inflexions of the epithelial layer, and represent in this animal the uterine mucosa. These follicles, he says (p. 146), develope largely during pregnancy, are transformed into a glandular organ, and appear destined to replace, during pregnancy, the utricular glands which are wanting in these animals. I have had the opportunity of examining sections through not only the non-gravid uterine mucosa of the rabbit, but through the non-placental portions of the mucosa in the gravid uterus, which have been carefully prepared by Mr Stirling, and, from the appearances presented by these sections, there is, I think, good reason to regard these "glandular follicles" as only utricular glands somewhat modified in their shape. For though the part which lies next the uterine cavity has not the cylindrical tubular form usually exhi-

[1] I am aware that Eschricht, in his important memoir (*De Organis*, &c., p. 24), speaks of the vessels in the feline placenta as exhibiting dilatations, and that Kölliker (*Entwickelungsgeschichte*, p. 163) states that in the bitch the maternal blood-vessels are very strongly developed, and appear as very thin-walled capillaries $\frac{1}{8}'''$ in breadth, but I have not seen in the placenta of this animal sinuses at all comparable with the sinuses in the sloth, which possess a transverse diameter of from ·003 to ·008 inch.

[2] *Beiträge zur Entwickelungsgeschichte des Meerschweinchens in Abhand. der König. Akad. der Wissensch. zu Berlin*, 1861.

[3] *Lehrbuch der Histologie*, p. 517. 1867.

[4] *Sur les Glandes Utriculaires de l'Uterus*, p. 10. French translation. Algiers, 1869.

[5] Op. cit. plates i. ii., p. 117, and Müller's *Archiv*, p. 80. 1848.

[6] Sulla le Glandole Otricolari dell' Utero. *Mem. del Acad. dell Sc. di Bologna*, p. 26. 1873.

bited by the utricular glands, the deeper extremities of these follicles lying next the submucosa present, on transverse section, a circular or oval form, and on longitudinal section an elongated tubular form, such as the utricular glands themselves exhibit. Moreover, both the dilated and tubular portions of the so-called follicles are lined by a columnar epithelium which projects into their cavity, but leaves a central lumen. It is clear, therefore, that these follicles or glands exist in the uterine mucosa prior to impregnation, and are not occasioned by the gravid condition.

The very important observations recently made by M. Alphonse Milne-Edwards[1], on the placentation of the Lemurs, furnish some material for comparing this group of animals with the sloths. M. Edwards has examined specimens of the genera *Propithecus*, *Lepilemur*, *Hapalemur*, and *Chirogaleus*. He found the chorion almost entirely covered by dense compact villosities constituting a sort of vascular cushion, the result of the confluence of a multitude of irregular cotyledons. The placenta had the appearance of a large sac which almost completely enclosed as in a hood the amnion. This form of placenta, he calls, bell-like (*placenta en cloche*), for the villi are most numerous at the upper and middle parts of the chorion, but almost entirely disappear towards the cephalic pole. The uterine mucosa corresponding to the villosities exhibited numerous irregular anfractuosities, and had developed a caducous layer. An enormous sac-like allantois was situated between the chorion and amnion. In its general mode of disposition on the chorion, the placenta of the lemur is not unlike that of the sloth, but in the latter animal the lobes or cotyledons are apparently less intimately blended than in the former, which has in addition a highly developed allantois; but as no information is given on the arrangement of the utricular glands or maternal blood-vessels, I can make no comparison between the disposition of these important structures in the lemurs and sloths[2].

In the new-world Monkeys the placenta consists of a single disc-shaped organ, which is probably also the case in the anthropomorphous apes, though in the tailed apes of the old world, as Hunter and Breschet's observations have sufficiently shewn[3], the placenta is

[1] *Annales des Sciences Naturelles*, xv., 1871.

[2] Since this Memoir has been put in type my attention has been directed by Prof. R. O. Cunningham, to a paper "On the Lemurs," by Mr St George Mivart, in *Proc. Zool. Soc.* London, May 20th, 1873. Mr Mivart states that from a private communication made to him by M. Alphonse Milne-Edwards, that naturalist is now of opinion that the Lemurs have no decidua, and that the placenta is diffuse. In does not appear from Mr Mivart's paper whether M, Milne-Edwards had received additional specimens since the publication of his Memoir quoted in the text, in which, when describing the uterine mucosa of *Propithecus*, he says, "Et la surface en est hypertrophée façon à former un couche caduque, très-analogue à celle qui, dans une tresfaible étendue, adhère au placenta discoïde des Singes, des Insectivores et des Rongeurs." From this extract it is clear that when his Memoir was written, M. Milne-Edwards had no doubt of the presence of a decidua.

[3] Figures of the placenta in the Quadrumana, or descriptions of its naked-eye characters, will be found in John Hunter's Collected Works, IV. 71; Plates XXXV., XXXVI. and fig. 2, XXXIV.; in Rudolphi's Memoir, "Ueber den Embryo der

subdivided into two large lobes by a greater or less interval. John Hunter has pointed out that each of these large lobes is made up of smaller ones, united closely at their edges—a feature which Breschet also has confirmed (*op. cit.* p. 445). The subdivision of the placenta in these monkeys into two parts is interesting in connection with the arrangement seen in the sloth's placenta, where a partial separation into a right and left lateral half was found, each of which in its turn consisted of smaller lobes. But though the form of the placenta, the existence of a decidua, the absence of the sac of the allantois, the absence, or at least the rudimentary condition, of the umbilical vesicle[1], the arrangement of the amnion, and the comparative length of the umbilical cord, have all been determined in the quadrumana, and correspond in most particulars with what I have described in the sloth, yet there is, unfortunately, a want of precise information on the arrangement of the maternal blood-vessels, and the condition of the utricular glands in the former group of animals. John Hunter, indeed, says, veins or sinuses were placed in the fissures between the lobes which received the blood laterally from the lobes, and that the substance of the placenta seemed to be "cellular," as in the human subject. Dr Rolleston, from the examination of a spirit-preserved placenta of *Macacus nemestrinus* (*op. cit.* p. 301), obviously inclines to the view that in it intraplacental maternal sinuses existed, though he points out that, from the age of the specimen, the examination was not so satisfactory as he could have desired. Prof. Ercolani states[2], from an examination of a specimen preserved in spirit, of the placenta of *Cercopithecus sabaeus*, that in this monkey the placenta in its microscopic characters, as well external as internal, does not present any difference from that of woman, so much alike are they, that as he had just described the human placenta, he did not think it necessary to go into the details of structure in the ape. He does mention, however, that the intraplacental lacunæ in *Cercopithecus*, which contain the maternal blood, are smaller than in woman; and that on the uterine face of the placenta are manifest traces of serotina, which is continued on to the fœtal villi forming the external membrane, or the walls of Ercolani's glandular organ. As he had stated on p. 36 that he had never seen utricular glands in the human placenta, we must assume from the similarity in structure that he had not observed them in this monkey.

As I could find no record of observations on the utricular glands, even in the unimpregnated uterus in the quadrumana, I examined the mucosa of a non-gravid spider-monkey, apparently the *Ateles gricescens*, preserved in spirit. I found numerous short glands, dilated

Affen," already quoted; in Breschet's important Essay in *Memoires de l'Institut*, 1845, xix.; and in Owen's *Comparative Anatomy of Vertebrates*, III. 747, in which volume it is mentioned that the pregnant monkey, dissected by Hunter, the name of which that anatomist had omitted to give, was a specimen of *Macacus rhesus*.

[1] Compare Breschet's description of *Simia Sabaea* with that of *Simia nasua*, pp. 444, 470.

[2] *Mem. dell' Acad. di Bologna*, 1870, p. 53.

into pouch-like recesses, and containing an abundance of epithelium, opening into the uterine cavity by constricted mouths. Some few which possessed the form of short tubes were interspersed amidst these pouch-like glands. In this ape, as in the human female, the mucosa was in close relation to the subjacent muscular coat, and was not attached to it, as in the sloth and mammals generally, by a lax coat of submucous connective tissue.

I owe to Dr Rolleston the opportunity of examining a slice of the placenta of the *Macacus nemestrinus* in the Oxford University Museum. Both to the naked eye and with a simple microscope, a section through the organ shewed a spongy character similar to that exhibited by the human placenta. The arborescent arrangement of the villi, and the processes of decidua, prolonged from the serotina into the interior of the placenta, corresponded to Dr Rolleston's description. I examined the bud-like offshoots of the villi microscopally; the capillaries filled with a yellow injection were arranged, not in loops, but in networks. Between these vessels and the periphery of the villus, a relatively thick layer of tissue intervened, which seemed to consist of two strata; one next the capillaries, which consisted of cells such as Ercolani regards as the glandular organ, continuous with the serotina; the other, and more external, was apparently composed of flattened cells which, on the supposition of an intra-placental circulation of maternal blood, and an involution of the utero-placental vessels, probably represents the wall of the maternal blood-vessels reflected on to the villus. A microscopic examination of the serotina displayed numerous large fusiform cells with ovoid nuclei, mingled with which were circular or spherical cells possessing granular contents and nuclei often of a globular form. The processes of the decidua contained fusiform cells smaller than those just described, and a nucleated protoplasm in which a differentiation into definite cell-forms was not clearly demonstrable.

The great attention which has been paid by various eminent anatomists to the structure of the Human placenta, enables one to institute a closer comparison between it and that of the sloth than could be done with regard to the quadrumana. In man the lobes are more closely fused together into a single organ, though the original subdivision into separate lobes is not unfrequently shewn by deep fissures extending from the uterine surface deeply into its substance, and in a case described by M. Breschet[1] the outer face of the chorion is said to have exhibited, not a regular placenta, but a number of distinct cotyledons. But, further, cases have been seen[2] in which the human placenta was divided into two not quite equal parts, a condition which is normal in the tailed monkeys of the old world, and an approximation to which, as I have already stated, is seen in the placenta of the sloth. Some recent observations by Reichert[3], on a young human embryo, at the twelfth or thirteenth day, have shewn

[1] *Répertoire Général*, p. 3, Paris, 1826.

[2] See Hecker, as quoted by Dr J. Matthews Duncan, in *Edinburgh Medical Journal*, Nov. 1873.

[3] *Reichert und du Bois Reymond's Archiv*, p. 127. 1873.

that, at this very early period, distinct, elevated islets or cotyledons had formed in the uterine mucosa, and a median cleft separated these islets into two halves, so as to give a bilaterally symmetrical arrangement. If, as is not improbable, these islets are the rudiments of the lobes of the future placenta, the human placenta approximates in its bilateral arrangement at this stage of development to what I have seen in the sloth.

Both in the human and sloth's placenta, curling arteries and utero-placental veins are present, though in the former their subdivision in the substace of the placenta into branches cannot be followed out as in the latter, in which animal, moreover, the dilatation of the veins into large sinuses within the muscular wall of the uterus and the serotina does not exist as in the human female. In both, intra-placental maternal sinuses, communicating on the one hand with the curling arteries, on the other with the utero-placental veins, are met with. In the sloth, however, these sinuses retain their individuality, their walls can be isolated from the adjacent villi, they have a tubular form, and their arrangement as an anastomosing network is preserved[1]. The mode in which they wind in and out between the villi bears a strong resemblance to the description and figures given[2] by Dr Priestley of the arrangement of the maternal vessels in a human embryo at the second month; only in his case the vessels, though described as "capacious capillaries," were not dilated into large sinuses as in the sloth. In the fully formed human placenta, on the other hand, though the walls of the curling arteries and utero-placental veins are distinct structures, yet the intra-placental maternal blood-sinuses are not tubular, but consist of a system of irregularly formed and freely communicating spaces. Whether they possess delicate walls separating them from the tissue of the fœtal villi, or whether the villi float naked in the mother's blood, are questions which have been much debated amongst anatomists. Every one who has examined the villi of the human chorion is familiar with the layer of nucleated cells which invests the villi like a cap. It has been repeatedly figured, and is seen to lie immediately outside the single or double capillary loop, which the bud-like offshoots of the human villi contain. Opinions are divided whether this layer of cells belongs to the villus, or is a layer of cells derived from the decidua serotina, which has become blended with the tissue of the villus; but whether we regard these cells as proper to the villus, or as belonging to the serotina, no corresponding layer was seen in the sloth. In the course of observations made during the past two years on the minute structure of the human chorionic villi, I have more than once seen an appearance which led me to believe that outside this layer of well-defined cells, *i.e.* nearer, or rather next to the maternal blood, was a layer of squamous cells, which would represent, therefore, the endothelium of the maternal blood-vessels, blended with the villus owing

[1] The arrangement in the sloth is, indeed, not unlike what E. H. Weber conceived to be the arrangement in the human placenta.

[2] *Lectures on the Development of the Gravid Uterus*, p. 62, figs. 19, 20. London, 1860.

to the great expansion of these vessels into the irregularly-shaped, freely-communicating blood-sinuses. In the sloth, again, as the intra-placental sinuses retain the individuality of their walls, their endothelium remains in its proper position as a layer of cells lining the maternal blood-tubes. In the sloth, therefore, the capillaries of the chorionic villi lie closer to the periphery of the villus than is the case in the human placenta; but, further, in the sloth these capillaries are arranged as a distinct network, whilst in man they form single or double loops. The branches which arise from the stems of the villi are more elongated, and have more of a laminated arrangement than is the case in the human placenta.

The presence of a system of maternal sinuses within the placenta of the sloth, though, as I have just pointed out, it differs in several points from the corresponding arrangement in man, is of great anatomical interest. For it presents a transitional form between the simple maternal capillary plexus met with in a diffused, cotyledonary, or zonary placenta, and the irregularly formed blood-spaces which exist in the placenta of the human subject. Several obstetrical writers have, from time to time, denied the existence of intra-placental maternal blood-sinuses in the human female, and the objection has been advanced that the presence of such sinuses receives no support from comparative anatomy. In a communication, read to this Society, May 20, 1872[1], I adduced a number of facts, derived from the study of the human placenta, in support of the Hunterian doctrine of the intra-placental circulation of maternal blood, and I may now add to these facts the confirmatory evidence afforded by the structure of the placenta in the sloth.

The condition of the utricular glands in the mucosa of the human and sloth's uterus is also a feature of much interest. In both, in the quiescent, non-gravid state, the glands are short, comparatively simple in form, and are not demonstrable with the same readiness as in the uteri of animals, which, in the gravid state, possess a diffused, a cotyledonary or a zonary placenta. In the human uterus, at the commencement of gestation, the glands are well marked, but as pregnancy advances to its middle period they disappear, so that no traces can be seen of them, either in the decidua or in the fully formed placenta[2]. Of the condition of the glands, in the early period of gestation in the sloth, we have no information, but that they are absent in the later period I have little doubt, for the careful examination to which I subjected both the placenta and the non-placental area of the mucosa would have disclosed them had they been present.

Both in the sloth and in the human subject the decidua reflexa is well marked. The serotina is not, however, so strongly developed in the sloth, more especially is there a deficiency in the granular colossal

[1] Abstract in *Proceedings* of that date, and more fully in *Journal of Anatomy and Physiology*, November, 1872.

[2] Dr Priestley states (p. 27), that he could see them distinctly in the parietal decidua near the seat of the placenta in the third month, but they were undergoing granular degeneration, and, instead of being lined with epithelium, were filled with granules and molecules.

cells, which in the human placenta not only lie on its uterine aspect, but pass into its substance in the decidual dissepiments.

The great importance in fœtal nutrition, which has been ascribed by Ercolani, in his several valuable and most instructive memoirs on placental structure, to a system of gland-follicles forming the maternal part of the placenta, which he believed to be new formed during pregnancy in all placental mammals, and in which the fœtal villi are lodged, naturally led me to examine the sloth's placenta with care, to ascertain if in it such gland-follicles could be recognised. I failed, however, to see any indications of follicular structure either in the defined form described by him to exist in the diffused, cotyledonary or zonary placenta, or as a layer of cells, belonging to the decidua serotina, investing the fœtal villi as in the human female.

I have already, in my memoir on the Placentation of the Cetacea[1], criticised and advanced some objections to the general applicability of Ercolani's theory, and from the study of the placenta in the sloth there appear to be additional reasons for doubting that anatomical unity in placental structure which he advocates. The presence of a gland-secretion as an osmotic medium in fœtal nutrition, whether we regard it as produced by new-formed gland-follicles, as Ercolani supposed, or by the utricular glands themselves, as Eschricht argued, does not, I believe, necessarily occur in all forms of placentæ. That a white fluid, subsequently termed uterine milk, which serves as aliment for the fœtus, is present in the cotyledons of the ruminants, was known to Harvey and the older school of physiologists, and it is very probable that a similar fluid is produced in all placentæ where uterine glands or follicles continue to secrete during the whole period of placental formation. But in those placentæ, as the sloth, the apes, and the human female, where an unusual development of the maternal blood-vessels into large sinuses takes place, a modification in the anatomical structure is introduced, which seems to render the presence of such a secretion unnecessary; the utricular glands seen in the non-gravid uterus disappear, no new-formed follicles are produced, and the nutritive changes, in all probability, take place directly between the fœtal and maternal blood.

The amnion in the sloth is related to the chorion, placenta, and umbilical cord, as in the human female. The sac of the allantois and the urachus have disappeared, and I could see no trace of an umbilical vesicle. Further, I may state that the uterus is simple and uniparous, and that the mammæ are two in number and pectoral in position.

In classifying the Sloths and the other members of the Order Edentata it has been customary for zoologists to rank them with the lower orders of mammals. Professor Owen[2], for example, directs attention to the supernumerary cervical vertebræ supporting false ribs, and the convolution of the windpipe in the thorax of *Bradypus*, as manifesting its affinity to the oviparous vertebrata, and to

[1] *Trans. Roy. Soc., Edinburgh*, Vol. xxvi. p. 467.
[2] *Reade Lecture On the Classification of the Mammalia*, p. 31, 1859.

the unusual length of the dorsal and short lumbar spine in *Cholopus* as recalling a lacertine structure; whilst the abdominal testes, single cloacal outlet, low cerebral development, absence of medullary canals in the long bones in the sloths, and long-enduriug irritability of the muscular fibre in both the Sloths and Anteaters shew the same tendency to an inferior type. In his system of classification, based on the cerebral characters, he places them in the group Lissencephala, along with the Rodentia, Insectivora, and Cheiroptera. Professor H. Milne-Edwards, in his most recent defence of his placental system of classification[1], whilst admitting the insufficiency of the information on the mode of development of the Edentata, considers that, from the structure of the teeth and the absence of incisors, these animals have affinities with the Cetacea more than with other mammals, though they appear to have some relations with the Monotremata, and he does not hesitate to form a separate phalanx for their reception. Professor Haeckel, whilst ranking them amongst the Indeciduata[2], admits that the genealogy of the Edentata is very difficult. Perhaps, he says, they are nothing but a peculiarly developed offshoot of the Ungulata, but perhaps their root may lie in a very different direction.

The comparison which I have just made between the placenta of the sloth and that of the other deciduate mammals reveals a correspondence in important features, both of arrangement and structure, between the placenta of the sloth, that of the human female, and of the monkeys, greater than exists between it and the same organ in any of the other orders of the Deciduata, so far as has yet been described. This correspondence in placental form and structure between mammals, which on general zoological grounds are so widely separated, affords room for much speculation and thought, and throws a new light, not only on the position of the sloths in the order Edentata, but on their relations generally to the placental mammals.

Professor H. Milne-Edwards, in the Memoir above referred to, argues that similarity in the form of the placenta and in the arrangement of the membranes is associated with resemblances in other important structural characters, so that the classification of mammals founded upon the placenta rests on a natural basis. Thus Man, the Quadrumana, Cheiroptera, Insectivora, and Rodents, are grouped together by him in the Micrallantoid legion of the phalanx *Hématogénètes*, as they possess in common a discoid placenta, a small allantois, and a caduca uterina. But further, they are all markedly unguiculated, their teeth are provided with a covering of enamel, and the dental series is continued around the front of the jaws.

As regards their placental characters the Sloths would fall into this Micrallantoid legion, with which also they would be associated

[1] *Considérations sur les affinités naturelles et la classification methodique des Mammifères*, being the first chapter in the *Recherches pour servir à l'histoire naturelle des Mammifères*, now in course of publication by himself and his son M. Alphonse. Preface, dated 27th April, 1868. Paris.

[2] *Natürliche Schöpfungsgeschichte*. Berlin, 1868, p. 480.

by their long claws; but in the structural characters of their teeth and the absence of incisors, they are at once markedly distinguished from them, so that in these respects the correspondence between placental form and structure, and these other well pronounced natural characters, breaks down.

Between *Cholopus* and *Homo* the divergence in most of their organic systems is so great that it is difficult to find evidence of any affinity except in their placental characters. With the Prosimii and Apes, however, affinities may be found. De Blainville had indeed many years ago[1] indicated correspondences in the skeleton of the Sloths and the Apes, more especially the Gibbons; I may here refer to the very remarkable vascular plexuses which exist in the limbs both of the Sloths and Lemurs; and now that I have called attention to the evidences of affinity with these higher mammals it is not improbable that other features of resemblance may in time be recognised. From the point of view of those who hold the descent-hypothesis it is possible that between the Sloths and the Lemurs genealogical relations may exist.

In conclusion, I may state that the study of the placenta in the Sloth has shewn how difficult it is to predicate, from the arrangement and structure of the other organic systems, what the character of the placenta may be, and how necessary it is, before a proper estimate can be formed of the nature of the placentation, not only that the form of the organ and the arrangement of the membranes in the different orders of mammals should be worked out, but the modifications in its minute structure should also be determined. Moreover, it would seem that affinities in placental form and structure may exist between mammals which in many other respects are widely separated, so that the placenta is not in itself sufficient to determine the position of an animal in the mammalian series, and the use of this organ as a basis of classification, though in many instances it may be relied on, yet, from the complex cross-relations which exist between the several organic systems in the placental mammals, is not universally applicable.

[1] *Ostéographie des Mammifères.* Paresseux, p. 1.

NOTES OF SOME MUSCULAR IRREGULARITIES. By JOHN CURNOW, M.D., *London; Professor of Anatomy in King's College, London.*

SINCE the publication of my paper in the June number of the *Journal* for last year, 42 bodies have been dissected at King's College, and in every one, without an exception, some deviation from the normal arrangement of the muscles has been found. Although they were most frequent in the more muscular bodies, yet important abnormalities were observed in many very thin subjects, whilst in an excessively muscular male, they were quite insignificant. I am therefore inclined to attach less importance to the mere muscularity of the individual than many anatomists seem to do. I have selected from amongst my notes some of the rarer forms of irregularity in the muscles of the neck and trunk, reserving those of the limbs for a future opportunity.

1. *Cleido-occipital.* Besides several instances of the usual form of this muscle, it was twice made up of a sternal as well as of a clavicular factor. Both were muscular throughout, and on the left side only, while the usual single variety was present on the right side. In the first case, the inner head arose from the anterior surface of the manubrium sterni, just internal to the first costo-sternal articulation, distinct from, and external to the sternal portion of the sterno-cleido-mastoid, and the outer head arose from its usual position on the clavicle a little external to the clavicular portion of that muscle. The inner fibres crossed the sterno-mastoid superficially, and joined the outer about the middle of the neck, forming a large muscle quite two inches in width. The *sterno-cleido-mastoid* was also very irregular, for its two parts were almost entirely separate, a few fibres only of the clavicular portion uniting with the sternal within an inch of the mastoid process. Its sternal origin was split into three divisions, for, in addition to the normal attachment, two slips passed down from its inner border over the manubrium—one to take origin from the lower half of that bone, and the other to become continuous with a well-marked rectus sternalis which was also present.

In the second case, the sternal origin of the cleido-occipital was exactly similar, but its clavicular head was divided into two parts by an areolar interval one inch wide, and extending upwards for two inches. The sterno-cleido-mastoid in the middle third of the neck gave off a cross slip from its inner (sternal) portion to the cleido-occipital.

Two doubled-headed specimens of this muscle are described by Prof. Wood (*Phil. Trans.* 1870), and its relations to similar forms in several animals is pointed out, while a third example is recorded by Mr Bradley in Vol. VI. of this *Journal.*

2. *Rectus Sternalis.* (*Rectus thoracicus superficialis.*) This muscle was seen in three subjects; in one on both sides, and in the others on the right side only. The body in which it was double is the same as that in which the first cleido-occipital occurred. On both sides

the muscles were well developed, one inch wide, crossed the thin sternal fibres of the pectoralis major superficially,—the left extending from the *above described* tendinous prolongation of the mesial tendon of the sterno-mastoid, and from the adjoining surface of bone for a slight extent outwards, to the level of the seventh costal cartilage, where it became attached to the anterior lamina of the sheath of the rectus abdominis. The other specimens were less complete, for neither of them passed upwards to the sterno-mastoid, and one only extended downwards as far as the upper margin of the fifth rib-cartilage. There were no tendinous intersections.

3. *Supra-costalis.* (*Rectus thoracicus profundus.*) A very singular form of this muscle was found on the left side of a male subject. It was attached to the upper border of the third rib, just internal to the origin of the serratus magnus, and passed upwards behind the pectorals and clavicle, over the upper two ribs, lying partly on the inner margins of the digitations of the serratus. It continued along the outer border of the scalenus medius, slightly overlapping it, and was crossed superficially by the subclavian vessels and the cords of the brachial plexus, while the external respiratory nerve of Bell appeared just behind it. It then crossed inwards over the scalenus medius, giving off a very few fibres thereto, and ended in a strong but thin tendon, which joined the *scalenus anticus* at its origin from the anterior tubercle of the fourth cervical vertebra. The scaleni were quite normal in every other respect. In most instances the supra-costalis either stops at the first rib, or is lost in the fascia over the lower part of the scaleni; but two cases in which it was blended with the middle scalenus are on record; viz. one by Lawson Tait in this *Journal* (Vol. IV. p. 236), and another by Pye-Smith in *Guy's Hospital Reports* for 1871. I have found no notice of such an intimate connexion with the anterior scalenus as is above described.

4. *Omo-hyoid.* Although this muscle is so very variable, yet cases of complete duplicity are very infrequent. It occurred on the left side only of a thin, aged female. Both muscles were digastric, with an intervening tendon. The more anterior muscle was attached to the upper costa of the scapula, from half an inch behind the notch to the angle, and followed the usual course of the normal omo-hyoid to the hyoid bone. The other muscle was attached to the ligament over the supra-scapular notch and reached backwards a little behind the origin of the former. It passed forwards along the posterior border of, and then a little above the clavicle (to which the tendons of both were bound down by the cervical fascia), and the anterior belly joined the sterno-hyoid about its middle. The anterior bellies of both were of much the same size, and were larger than the posterior; the difference being especially marked in the lower or supernumerary muscle.

5. *Sterno-thyroid.* An additional sterno-thyroid is more common than the preceding, and I noticed it once also. It was half an inch wide, and lay internal to the usual muscle.

6. *Crico-hyoid.* On the left side of a male larynx, a bundle of muscular fibres, one-third of an inch in width, extended from the

upper border of the cricoid cartilage to the lower border of the body of the hyoid bone, just internal to the greater cornu. It arose internally to the crico-thyroid and ran up near the median line, being separated by a very distinct interval from the inner border of the thyro-hyoid, with which it was parallel. The only reference to this muscle is by Zagorsky, in 1806, who gave it the above name. In the case described by him it occurred on both sides.

7. *Digastric.* From the junction of the posterior belly with the tendon, a muscular slip, two inches long and half an inch wide, passed downwards and inwards over the styloid muscles and the middle constrictor of the pharynx, and was blended with the upper edge of the inferior constrictor, a little external to the thyroid cartilage. The styloid muscles were quite normal.

In another subject a thin digastric muscle (which I looked on as an abortive *Occipito-hyoid*), was attached behind to the fascia on the superficial surface of the splenius capitis near its insertion. It crossed the sterno-mastoid lying beneath the platysma and was lost on the deep cervical fascia between the carotid sheath and the hyoid bone. It was present on the right side only. Fasciculi somewhat similar to these are described by Perrin in Vol. v., and by West in the last number of this *Journal.*

8. An additional small muscle, triangular in shape, was once seen in the sub-occipital triangle. It arose from the posterior tubercle of the atlas, close to the rectus capitis posticus minor, and crossing between the recti muscles was inserted into the occipital bone external to the greater rectus and under cover of the superior oblique.

9. *Rectus abdominis.* On both sides of a moderately muscular male, the posterior division of the sheath of this muscle ceased abruptly about an inch below the umbilicus, and from this point to near the pubis, a large quantity of *unstriped* muscular fibres lay on, and was intermixed with the fascia transversalis, which was less developed than usual. The fibres were principally transverse, but some crossed superficially in every direction, and their appearance forcibly reminded one of the bladder after its peritoneal investment has been stripped off. They became gradually indistinct externally, internally, and below, and did not reach the linea semilunaris, the linea alba, or Poupart's ligament, but above a few fibres were continued behind the sheath for a very short distance. The deep epigastric artery was immediately superficial to the muscular layer, having pierced the fascia transversalis very low. The character of the fibres was determined microscopically. I can find no description of such an arrangement, and Prof. Wood tells me that he has never seen unstriped muscle in this situation.

NOTES OF A DISSECTION OF AN EXCISED ELBOW.

By G. J. Malcolm Smith, M.B., *Demonstrator of Anatomy in the University of Edinburgh.*

Early in the present session a male subject, apparently about 65 years of age, was brought into the Dissecting Room of the University. On examination, before dissection was commenced, it was found that the right elbow-joint had at some former period been excised by the H method; and the result evidently was a flail joint, and an almost useless arm, proved by the atrophy of the limb, and the conditions on dissection.

As one seldom has a chance of examining the result of excision of this joint, I gladly availed myself of the opportunity afforded, and append the notes which I made of the dissection.

Examination before dissection.—The whole limb was atrophied, and presented a marked contrast to that of the opposite side. The cicatrix on the back of the elbow was in the form of the letter H, and apparently of considerable age. The forearm was shortened, the superior extremities of the radius and ulna resting on the front of the lower extremity of the humerus: it could not be flexed more than a degree or two past a right angle, but could be completely extended. There was very slight lateral motion. On rotating the forearm with one hand, with the fingers of the other hand manipulating the elbow-joint, it was found that *both* radius and ulna moved on the humerus, in fact that there was little or no independent motion of the head of the radius. Careful examination of the relative position and shape of the new joint was easy, on account of the atrophy and thinness of the limb, and of the absence of any new bone round the articulation. The humerus ended in two sharp projections, the outer extending one and a half inches lower down than the inner. The inner of these projections was posterior, inferior, and external to the upper end of the ulna. The breadth of the humerus at its lower extremity was one inch and a half. The upper end of the radius lay *in front of the inner projection of the humerus, and behind the ulna.* The ulna terminated superiorly in a process which ended in a sharp projection, and which lay in front of the outer projection of the humerus, separated from it by about half an inch of soft parts. The forearm was shortened and the upper ends of its bones were one inch and three quarters above the lower end of the humerus.

On *reflecting* the skin and fascia of the limb the *biceps* was found much atrophied (as were all the other muscles); its surfaces looked outwards and inwards, its inner border being turned forwards. The long tendons of insertion curved round the upper extremity of the ulna to be inserted into the upper end of the radius. The fibres of the brachialis anticus passed downwards and forwards, arising

from the whole of the external half of the anterior and lower aspect of the humerus, to be inserted into the apex of the superior projection of the ulna,—this projection then was without doubt the coronoid process. The triceps over the lower extremity of the humerus was very thin, and was inserted by fibrous bands into the ulna, two inches from its upper margin, and also into the fascia of the forearm. These fibres of the triceps formed the posterior ligament of the new joint. The anterior muscles of the forearm arose from the internal projection of the humerus; those of the posterior aspect arose from the external projection. These projections were evidently the remains of the condyloid eminences and ridges. With the exception of the biceps, triceps and brachialis anticus, the other muscles presented no peculiarity or deviation from normal arrangement worthy of description.

Arteries.—Brachial artery, tortuous on account of the shortening of the forearm; lay on coraco-brachialis, brachialis anticus, passed over the ulnar projection, then curving outwards lay on the radius and gave off the radial and ulnar arteries one inch below the upper border of the extremity of the ulna. The ulnar artery was twice the size of the radial, and after arising from the brachial, where it lay on the radius, pursued its usual course. The radial artery requires no description. There was a free anastomosis round the joint between the profunda, anastomotica, and recurrent vessels, which were all markedly enlarged.

The ulnar nerve, opposite the lower extremity of the humerus, was expanded and flattened out: the length of this expansion was three quarters of an inch, the breadth half an inch. This expansion was attached to the subjacent muscles and fibrous covering of the joint, and from it two nervous processes arose; one internal, and of the normal size of the ulnar nerve in this situation, was one inch in length, and was the upper cut end of the ulnar nerve; the other external, at first very thin and connected very slightly to the expansion, after about an inch became thicker and was continued down he forearm without any peculiarity. Round the extremities of the three bones was an imperfect fibrous capsule, with a small synovial membrane, resembling that of a bursa rather than of a joint. There was almost no cavity, the two synovial surfaces lying in contact opposite the lower extremity of the humerus, and upper and posterior surfaces of the radius and ulna. There was no separate or special articulation between the radius and ulna, and no sort of orbicular ligament. The radius, as before stated, inclined behind the ulna, the tendon of the biceps hooking round the latter bone. The fibres of the capsule were attached to the summits of the radius and ulna and to the humerus along the anterior aspect of the lower end of the bone: posteriorly, the fibres were mainly those of the triceps muscle.

The bones require but short notice, since they corresponded with what has been described as felt on external manipulation. The upper end of the radius lying between the other two bones was smooth, and the biceps tendon was attached to its anterior aspect

half an inch from the upper border: the sharp projection of the ulna had exactly the appearance of the coronoid process, and had the tendon of the brachialis anticus attached to it. The point of greatest interest was the entire absence of any new bone: the three bones had exactly the appearance of normal and healthy bones excised in a dead subject, save that the sections were everywhere rounded off, and the cancellated texture was covered in. The bones showed marked atrophy on their section a few inches above and below the joint.

NOTE ON AN UNUSUALLY LARGE RENAL CALCULUS. By J. A. RUSSELL, M.A. and M.B., *Demonstrator of Anatomy, University of Edinburgh.*

DURING the dissection of a female subject apparently above middle age, last winter, in the Anatomical Rooms of this University, the left kidney was felt to contain a calculus of large size; and on removing the kidney and making a more careful examination it was found that the pelvis of the organ was distended by the stone, which could also be readily felt in the calices. The sinus contained much fat surrounding the vessels, and the vein and wall of the pelvis were adherent where they came in contact. The kidney, which measured only $3\frac{1}{2}$ inches by $1\frac{3}{4}$ by 1 inch, was longitudinally bisected from hilus to outer border so as to display the calculus *in situ*. The calculus was now seen to form a cast of the interior of the pelvis, infundibula and calices of the organ, and its pointed lower end extended along the ureter as far down as on a line with the lower end of the kidney. The extreme length of the calculus was three inches, and it divided into three main branches, one of which was situated in each infundibulum. These branches gradually enlarged from the point of division, and attained their greatest size at the free extremity. That for the upper end of the kidney was longest and thickest, and that for the middle part smallest.

The calculus was slightly tuberculated and stained brown over the greater part of the surface, but over the remainder and internally it was white and presented a crystalline appearance. All over the surface were seen bright points, due to crystals of ammonio-magnesian phosphate, of which the chemical tests shewed the calculus to be mainly composed. The structure was dense, tolerably hard, and though brittle it was not easily crushed. As many of the papillary apices of the pyramids of the kidney presented a natural appearance, and the atrophy of the cortical substance, though very considerable, was not extreme, it is probable that a certain amount of urine had been secreted by this kidney up to the time of death.

The right kidney of the same subject was normal in size, and had a large cyst upon its surface.

SINGULAR MALFORMATION OF WRIST AND HAND.

By Edward Bellamy, Esq. F.R.C.S.

The rarity of fusion of the bones of the carpus has led me to place the following specimen on record. The body from which it was taken was that of a powerful middle-aged labourer, of great muscular development, and with no other noticeable irregularity. The os scaphoides was nearly normal as far as its surface articulating with the radius was concerned. The os lunare, cuneiforme, trapezoides, magnum and unciforme, were fused together into a quadrilateral mass articulating superiorly with the radius *and* the ulna, radially, with a tolerably normal scaphoid and trapezium, and on the portion of the mass corresponding to the os cuneiforme, with the os pisiforme. The unciform process was wanting in the mass, the articulating surfaces of which were very broad and continued far on to its dorsal and palmar aspects. The representative of the carpo-metacarpal articulation existed between the anterior surface of the aforenamed mass, the anterior surface of the representative of the trapezium, and three peculiarly shaped digits. The trapezium carried a fairly normal pollex, the phalanges of which were of great length and massiveness. The anterior articular surface of the fused mass carried two metacarpal bones, the inferior articular extremities of which corresponded almost entirely to the inferior articulating extremities of (1) the index and middle fingers, (2) the ring and little fingers. The phalanges were again enormously large. There was a congenital dislocation of the elbow-joint, which frequently occurs associated with a malformation of the hand, flexion and extension of which, however, appear to have been tolerably complete, the acquired articulating surfaces being very large. The fore-arm was capable of limited pronation, but not of supination. I regret very much that I am unable to give any account of the musculature of the hand, as the specimen came into my possession too mutilated to examine it effectually. The arrangement of the grooves over the posterior aspect of the radius leads me to imagine that the movements of this abnormal thumb were very limited, and an accurate description of the muscles and their tendons would have been of considerable myological interest.

NOTICES OF BOOKS.

Catalogue of the Preparations of Comparative Anatomy in the Museum of Guy's Hospital. By P. H. PYE-SMITH, B.A., M.D. London, 1874.

UNDER the unassuming title of a Catalogue of Preparations, Dr. Pye-Smith has compiled not only a useful descriptive catalogue of the comparative anatomical specimens in the Guy's Hospital Museum, but a work which may serve as an introduction to the science of comparative anatomy. He prefaces the catalogue with an introduction, in which he points out the leading characters and the principles of classification of the animal kingdom, and the distribution of animals both in space and time. The vertebrate specimens are arranged in physiological series, and an abstract of the leading characters of each class and order is prefixed to the description of the corresponding specimens. The invertebrate specimens again are arranged in zoological order, and are in a like manner prefaced by a short but succinct statement of the characters of the different classes and orders. With this book as his guide, the Guy's student is in a position to obtain not only a knowledge of the specimens in the museum, but an intelligent conception of the principles of anatomical science.

Animal Physiology: the Structure and Functions of the Human Body. By JOHN CLELAND, M.D., F.R.S. London and Glasgow, 1874.

The Students' Guide to Zoology, a Manual of the Principles of Zoological Science. By ANDREW WILSON. London, 1874.

WERE anything needed to prove how important a part the study of the biological sciences is beginning to play in the general education of the country, the publication in various quarters of so many manuals, which have for their object to popularize these sciences, would be a sufficient testimony. Dr Cleland's little manual is produced by the firm of Collins and Sons, as one of their Advanced Science Series. In preparing the work, the author tells us that he has kept constantly in view the desire of the publishers to supply the information required for the advanced course of the directory of the Science and Art Department. As might be expected from so able an anatomist and experienced a teacher as Dr Cleland, this book furnishes not only a lucid exposition of the current facts and opinions on human anatomy and physiology, but not unfrequently groups these facts together in a new and more instructive manner than had previously been done, and draws fresh and original conclusions from them. The chapters on the Nervous System and on Reproduction and Development may especially be referred to in illustration of the author's independence

of mind, when matters of opinion are under consideration. We cordially recommend this book to the notice not only of the general student, but to students of medicine, as an excellent and useful compendium of physiological science.

Mr Wilson's book is published by the firm of Churchill, London. Though called a guide to zoology, it is not so much a guide to the principles of zoology, in the restricted sense in which it is now customary to use that term, as a guide to the general Principles of Biology, and it would have afforded a better conception of the character of the book if the author had given it that name. The book does not pretend to discuss the various biological questions of which it treats from an original point of view, but is intended to give in plain and intelligible language an account of the opinions which are entertained on the leading biological questions of the day. The author has succeeded in writing a readable book, one in which the student may find much from which he may derive interest and instruction.

A Monograph of the British Annelids. Part 1. *The Nemerteans.* By W. C. M'Intosh, M.D. Printed for the Ray Society, 1873—1874. London.

Amongst the various important monographs by British naturalists which have been published by the Ray Society, none exceeds, either in the beauty of its illustrations or the careful preparation of the text, that on the British Nemerteans, by Dr M'Intosh, which the society has presented to its subscribers for the years 1873 and 1874. The monograph contains chapters on the habits, food, anatomy and physiology of this group of Annelids: a chapter on the reproduction of lost parts, and another on the parasites which infest them. Their classification, homologies and general distribution are also described, and an account of the several genera and species is given. Every page of the book exhibits the care which the author has taken in its preparation, and the desire he has shown to make it worthy to hold its place in the series of important monographs produced under the auspices of the Ray Society.

Grundriss der Vergleichenden Anatomie. Von Carl Gegenbaur. Leipzig, 1874.

Professor Gegenbaur's large work on Comparative Anatomy, the *Grundzüge*, is doubtless well known to most of our readers. As it treats the subject with an amount of detail greater than is needed by those who desire only to obtain a general acquaintance with the science, the author has been induced to prepare a shorter and more elementary work which he names the *Grundriss*. This book is not, however, a mere abstract of the larger work, but exhibits many modifications in the mode of treating the subject. As a general exposition of the principles of comparative anatomy, it is worthy of the reputation of its distinguished author.

REPORT ON THE PROGRESS OF ANATOMY.[1]

By Professor Turner.

Osseous System.—Wenzel Gruber gives in *Mém. de l'Acad. Imp. de St Pétersb.* 1873 a minute description, with illustrative figures, of Bones situated in the frontal Fontanelle. He has seen 43 cases in the 10,000 human crania which, in the course of 25 years, have been macerated in the Institute for Practical Anatomy. He also gives an example in a hydro-cephalic skull, and refers to specimens seen in the skulls of mammals. In the *Memoirs* of the same *Acad.* 1874, Gruber describes and figures a series of crania, both in man and other mammals, in which the Squamous-temporal articulated with the Frontal. He relates two modes in which the articulation may take place in the human skull: *a*, by direct union of the two bones which he has seen in only two crania; *b*, by the prolongation of a process from the temporal to the frontal. He has seen this in 58 human crania. In those mammals in which these bones articulate, the direct union occurs much more frequently than through the intermediation of a process.——In *Reichert u. du Bois Reymond's Archiv*, 1873, p. 337, Supernumerary Bones in the Zygomatic Arch are described by Gruber, and on p. 348 modifications in the Dental Foramen and Mylo-hyoid Groove in the lower jaw.——Th. Simon in *Virchow's Archiv*, lviii. 572, makes some observations on Persistence of the Frontal Suture. He found this suture 76 times in 809 cases, *i. e.* nearly 10 per cent.; the skulls were from persons between 10 and 100 years old. Of the 809 skulls 452 were males, 357 females, the percentage of cases of persistent suture being 8·4 in the men and 10·1 in the women. As a general rule the other sutures were well marked. In some cases the frontal suture was in part obliterated. As a rule the frontal is not continuous with the sagittal suture, but begins either to its right or left.——S. M. Bradley makes observations on the National Characteristics of Skulls in *Mem. Lit. & Phil. Soc. Manchester*, v. 213. He argues against Retzius's mode of classifying crania. He states that the skull-forms of (probably) every living nation range from extreme types of dolicho-cephalism to extreme types of brachy-cephalism, and constantly present examples of every intermediate form. His results were obtained by measuring the outline of the living head in some hundreds of persons with an apparatus used by hatters. He reproduces many of the outline figures to illustrate the paper.——T. Zaaijer describes (*Nederl. Tijdschr. v. Geneesk.* 1874) a Scaphocephalic Cranium and the head of a scaphocephalic man.——Fr. Merkel gives an account of the

[1] To assist in preparing the Report Professor Turner will be glad to receive separate copies of original memoirs and other contributions to Anatomy.

FEMUR (*Virchow's Archiv*, LIX. 237). He describes its position in the body; its external form; the direction in which weight presses on it, and its internal structure.——A. Kölliker continues (*Verh. d. Phys. Med. Gesellsch. Würzburg*, 1873) his observations on ABSORPTION OF BONE and INTERSTITIAL GROWTH OF BONE (*Reports* May and November, 1872). It principally consists in an adverse criticism of the observations of Strelzoff on the same subject, who, in the course of his researches into the histo-genesis of bone, had come to conclusions opposed to those of Kölliker.——J. v. Rustizky in *Virchow's Archiv*, LIX. 202, relates his observations on the ABSORPTION OF BONE and on giant-cells. He acknowledges the great importance of Kölliker's observations on the same subject referred to in *Report* May, 1872, but he does not attach the same importance to the giant-cells, as he considers that absorption of bone may go on without the intermediation of these colossal cells.——C. Robin describes in his *Journal*, 1874, 35, some comparative observations on the MARROW OF BONES.

TEETH.—C. Legros and E. Magitot consider, in *Robin's Journal*, 1873, 449, the ORIGIN AND FORMATION OF THE DENTAL FOLLICLE IN THE MAMMALIA. Their conclusions are: 1. The first indication of the dental follicle is a *cordon* arising from the epithelial layer of the mucous membrane of the gum. 2. The *cordon* which gives rise to the milk-follicles springs directly from a prolongation of the buccal epithelium, whilst that for the permanent teeth is a diverticulum from the primitive *cordon*. When the permanent teeth are not preceded by temporary teeth, they arise sometimes from the mucosa, sometimes from the *cordon* of the preceding molar. 3. The *cordon* is invariably epithelial; its peripheral part consists of the prismatic elements of the epithelial layer, its centre of polyhedral cells. 4. The extremity of the *cordon* constitutes the enamel-organ. 5. The dentary bulb appears spontaneously in the midst of the embryonic tissue close to the enamel-organs. 6. The enamel-organ is moulded like a hood on the dentary bulb. 7. The wall of the follicle is directly produced from the elements of the bulb, from the base of which it rises over the sides and summit of the follicle to constitute the sac of the follicle. 8. When the follicle closes the epithelial *cordon* is ruptured, and the follicle then loses its connection with the mucosa. 9. The phenomena of evolution of the follicle, either in the milk or permanent dentition, are the same. 10. The genesis of a tooth-follicle and a hair-follicle are identical.——C. S. Tomes describes (*Quart. Journ. Mic. Sc.* Jan. 1874) the EXISTENCE OF AN ENAMEL-ORGAN IN AN ARMADILLO. In *Tatusia Peba*, a mammal which has no enamel on its teeth, the first histological structure recognisable in the tooth-germ is a well-developed enamel-germ, and the enamel-organ in its teeth has an arrangement similar to that attained to by the enamel-organs in the teeth of mammals which possess a distinct cap of enamel. The presence of an enamel-organ in the early stage of development of teeth on which a cap of enamel does not form, had however been previously described by W. Turner in the Narwhal, in a paper published in this *Journal*, November, 1872.

RESPIRATORY SYSTEM.—E. W. Collins laid before the *Roy. Irish Acad.* April, 1874, a specimen illustrating an ACCESSORY LOBE OF THE RIGHT LUNG, taken from the body of a male subject, aged about 50 years, in the dissecting-room of the University of Dublin. The specimen presented a three-fold morphological peculiarity as regarded lung, pleura, and azygos vein. An accessory lobe, measuring four inches in length by two and a half in breadth at its widest portion, sprang from the angle between the root and the upper portion of the right lung, immediately above the bronchus. It was somewhat pyriform in shape, and rested upon the right side and front of the bodies of the five upper dorsal vertebræ, in an accessory pleural pouch. The pouch was formed by a duplicature, which depended from the cone of the pleura. It was continuous with the costal pleura externally along a line corresponding to the heads of the five upper ribs, internally along the mesial line of the five upper dorsal vertebræ. Between these points it arched over the accessory lobe, and isolated it from the remainder of the lung. It extended underneath the trachea, and invested the right side of the œsophagus. The azygos vein, instead of arching over the bronchus, arched over the peduncle of the accessory lobe, lying in the lower free margin of the pleural duplicature. This was the only specimen of the kind that had been noticed in Dublin, though seven similar cases had been recorded elsewhere. Stress was laid upon the suggestion of Prof. Cleland, made in this *Journal* (May, 1870), that an abnormal course of the azygos vein during the process of its development, whereby it drew down around it a pleural fold, and thus isolated a probably adherent portion of the lung, offered a satisfactory solution of the mode of formation of this accessory lobe. The author pointed out that a remarkable confirmation of this theory was to be found in an unique case recorded by Wrisberg of a similarly situated accessory lobe of the left lung, where the left azygos or superior intercostal vein preserved its fœtal condition by opening into the left vena innominata. The author regarded such a case as that described by Pozzi (*Report*, VIII. 174), and a similar one on the left side by Rektorzik, as merely higher developments of pulmonary notches. The paper concluded with an allusion to accessory bronchi in their connection with this subject.

BLOOD-VASCULAR SYSTEM.—C. Giacomini described to *Accad. di Med. di Torino*, Nov. 1873, a case in which the PORTAL AND RIGHT ILIAC VEINS freely communicated with each other.——W. Macdonald has published a pamphlet in which he explains his peculiar views on the SOURCE AND COURSE OF THE CIRCULATION in the embryos of warm-blooded vertebrates (Edinburgh, 1874).——A. Sabatier records his observations (*Ann. des Sc. Nat.* 1874) on the TRANSFORMATIONS OF THE AORTIC ARCHES in the vertebrata.

LYMPH-VASCULAR SYSTEM.—The researches of Recklinghausen, of Ludwig, and his pupils have of late years added much to our knowledge of the lymphatics, more especially in their relations to the serous membranes. E. Klein has just published an important

monograph ON THE SEROUS MEMBRANES, London, 1873, in which their structure and relations to the lymphatics are described with great care and illustrated by numerous beautiful plates. In chapter I. the endothelium of the free surface of these membranes is described more especially in connection with the presence of individual polyhedral, club-shaped, or even short columnar cells, with granular contents, an ovoid or sometimes spherical distinct nucleus, with large shining nucleolus: these cells he names germinating endothelium. In the frog they showed amœboid movements. In chapter II. the cellular elements of the ground-substance are described. In the omentum of the rabbit two kinds of *lymphangial* structures are recognised; *a.* patches, the matrix of which consists of groups of ordinary more or less flattened, more or less branched cells, which on the one hand multiply by division, so that the patch increases in size, and from which on the other hand grow up lymphoid cells. The branched cells lie in the lymph-canalicular system together with the lymphoid cells. At an early stage of development these patches do not contain a special system of blood-vessels; at a later they are especially rich in capillary blood-vessels; by growing in length these patches join so as to form whole tracts; *b.* patches and tracts the matrix of which consists of a reticulum the meshes of which contain a variable number of lymphoid corpuscles: they are generally provided with more or less abundant blood-vessels. In the omentum of the guinea-pig, cat, dog and monkey similar lymphangial structures are met with, but from their greater thickness they are named nodes or nodules. Intimate relations exist between these lymphangial structures and the fatty tissue of the omentum, the former indeed may and does become converted into the latter, and with this conversion the lymphoid cells diminish in number in the structure. He agrees with Fleming in saying that fat-cells are transformed branched cells. In chapter III. he describes the lymphatics of the serous membranes; some remarkable relations of the lymph to the blood-vessels are pointed out, more especially the invagination of the veins by lymphatics, and capillaries hanging in a lymph-sac. A growth of a network of branched cells may proceed from the endothelium of a lymph-sac, which may transform the sac into a cavernous structure in which lymph-corpuscles lie and fill up the meshes. Hence arise endo-lymphangial nodules, and as the proliferation increases the nodule extends more and more along the lymphatic vessel invaginating or accompanying a large blood-vessel. The relations of the lymph-vessels to the surface of the serous membrane are described in this chapter. Two kinds of stomata are recognised, stomata vera and pseudo-stomata. The stomata vera are of two kinds, *a.* the mouth of a vertical lymphatic channel, which is lined by a special layer of endothelium, and which channel leads into the lumen of a superficial lymph-vessel; *b.* a discontinuity between the endothelial cells of the surface, leading into a simple lymphatic sinus near the surface, which represents a cavity lined only on one side with an endothelium. Both kinds of stomata vera are bordered by endothelial elements of a more or less distinct germinating character, so that the mouths may be recognised

by the endothelial cells surrounding them differing in appearance from the ordinary endothelial cells. The lymphatic capillaries and the blood-vessels are then described. The book concludes with an important chapter on the pathological conditions of the serous membranes. E. Klein makes a communication to the Roy. Soc. London (*Proceed.* Jan. 29, 1874) on the LYMPHATICS OF THE LUNGS. He points out that the endothelium of the pleura pulmonum consists of polyhedral or shortly columnar, granular cells with very marked nuclei, while the pleura costarum has flattened, almost hyaline, endothelial plates. Beneath the endothelium of the pleura pulmonum is a very thin connective tissue membrane with numerous elastic fibres and a layer of flattened connective tissue corpuscles. In the guinea-pig a layer of non-striped muscular fibres lies beneath the proper pleural membrane, in rats, rabbits, cats and dogs, bundles of unstriped muscle-fibres occur sparingly. In chronic inflammation these bundles increase so as to form a continuous membrane. A system of inter-muscular lymphatics lined by an endothelium lies in the meshes of the muscular layer and communicates freely by stomata with the pleural cavity. A network of anastomosing lymph-vessels lies in grooves which correspond with the most superficial groups of alveoli of the lungs, which communicates on the one hand with the inter-muscular lymphatics, on the other with deeper inter-alveolar lymphatics. Lymphatic capillaries arise in the alveolar septa from branched cells and lead into lymph-vessels which accompany branches of the pulmonary artery and vein; they run either in the adventitia of these vessels, or the blood-vessel is entirely or only half invaginated in a perivascular lymph-vessel. The branched cells send a process between the epithelial cells into the cavities of the alveoli, which processes Klein names pseudo-stomata. Other lymphatics are distributed in the adventitia of the bronchi, thin capillaries originate in the branched cells of the mucosa of the bronchi and penetrate through the tunica muscularis. The branched cells penetrate between the epithelial cells of the mucosa and project on its free surface. Vascular lymph-follicles are in continuity with the endothelial wall of a lymph-vessel so that they are surrounded by it, just as a lymph-follicle of a Peyer's patch is surrounded by a lymph-sinus. The paper concludes with observations on the pathological conditions of the several structures. ——Fr. Tourneux communicates to *Robin's Journal*, 1874, 66, his observations on the EPITHELIUM OF THE SEROUS MEMBRANES. He regards the distinction recently drawn between epithelium and endothelium as arbitrary. His observations were made on batrachians. ——G. Thin describes and figures (*Lancet*, Feb. 14, 1874) a LYMPHATIC SYSTEM IN THE CORNEA. He has seen endothelial lined lymph-vessels traversing its substance, the endothelial cells possessing precisely the form of the endothelial lining of the lymphatics. He regards these vessels as the corneal tubes of Bowman and believes that the mercury in Bowman's injection had passed along these vessels. He describes lacunæ in the cornea in which the cornea-corpuscles lie, which lacunæ communicate with each other and with the lymphatic system. These lacunæ are lined by an endothelium

which is directly continuous with that lining the lymph-vessel. He states that nerves lie in the lymph-vessels and nearly fill them, a narrow space only being between the nerve and the wall of the lymphatic.—ON THE LYMPHATICS OF THE NORMAL NON-PREGNANT UTERUS. Dr G. Leopold has arrived at the following conclusions with regard to the lymphatics of the normal non-pregnant uterus (*Archiv für Gynäcol.* Vol. VI. Abst. in *Lond. Med. Rec.* I. 35).

I. *Mucous Membrane.*—1. The mucous membrane consists of a framework of the finest connective tissue, the bundles of which are covered with endothelium, and whose interspaces form the lymph-spaces (*Lymphräume*). 2. In the deeper layers, the membrane of the glands consists of a fine layer of delicate connective tissue bundles, whose epithelium lies externally, but more superficially it is formed only of a sheath composed of the cell-plates (*Zellplatten, plättchenförmigen Zellen*). 3. The blood-vessels, from the finest capillaries onwards, have a number of fine endothelial sheaths increasing with their size. 4. The framework of connective tissue stands by means of fine twigs in direct connection with both sorts of sheaths. 5. The glands and blood-vessels, therefore, pass directly through the lymph-spaces, separated only from the latter by their sheaths, formed from the framework of connective tissue. 6. At the limits of the muscular layer the lymph-spaces reach a short distance into the filter-shaped hollows between the muscular bundles, and become gradually narrowed into the intermuscular lymph-vessels and spaces.

II. *Muscular Coat.*—1. The muscular layer contains in animals and in the human subject lymph-vessels and lymph-spaces (*Lymphspalten*). The walls of each are formed of the fine intermuscular connective tissue. The former are lined by fine endothelial lamellæ, which exhibit here and there openings and slits; the latter are lined by delicate cell-plates (*Zellplatten*). 2. In animals, the characteristic net of lymphatics is arranged parallel to the long axis of the two layers of fibres; they therefore cross each other. Those of the inner layer pass into the lymph-spaces of the mucous membrane, and those of the outer into the subserous lymph-vessels. The large lymph-collecting tubes, provided with valves, and spread in the form of a net over the horns of the uterus, lie between the muscular layers, and receive the whole lymph-vessels from both sides: externally those of the subserous and outer muscular layers, and internally those of the inner layers and the mucous membrane. 3. In the uterus of the human subject the lymph-vessels are much more complicated, on account of the arrangement of the muscular fibres. They are most richly developed in the outer layer, and in the other layers specially in the neighbourhood of the large vessels, and are connected with the subserous membrane as in animals, but with the mucous more by lymph-spaces. They come together in the outer layer, especially at the side of the uterus. 4. The lymph-spaces, in the human subject and in animals, surround the smaller bundle of a large muscular bundle, and pass into the lymphatics. In animals, they stand in indirect connection with the subserous and mucous channels; in the human subject, however, in direct connection. 5. For the most part,

large blood-vessels lie in the neighbourhood of the large collecting tubes; the other lymph-vessels are partly accompanied by blood-vessels for a certain distance, and the lymph-spaces are almost regularly penetrated by small vessels.

III. *Serous Coat.*—1. Under the serous membrane only lymph-vessels are found. They lie in the subserous connective tissue, and form large characteristic nets. 2. They are much less numerous than the subserous blood-vessels lying over them, but are from eight to ten times stronger than the latter. 3. They have large ampullæ, points of union, constrictions, valves, and swellings, and send branches towards the deeper parts, either in a vertical direction or at an angle. 4. In the pig, rabbit, and sheep, the net has mostly a direction corresponding to the long axis of the uterine horns. In the human subject, on the contrary, they cover the anterior and posterior wall, in irregular large or small groups, and have, especially at the insertion of the Fallopian tube, large ampullæ, and then pass as an extended net upon the tube.

Nervous System.—In *Schultze's Archiv*, 1873, p. 208, Rudolf Arndt gives an account of his researches into the Ganglia of the Sympathetic. All the ganglion-bodies (cells) of the sympathetic are provided with several processes, indeed all bipolar and multipolar bodies correspond with complex cells, and are derived from complex cells. All unipolar ganglion-bodies, on the other hand, correspond to simple cells and proceed from them. All so-named apolar ganglion-bodies, when they are large, represent anomalous forms of development of the original formative cells; when they are small, they are the formative cells themselves.——In the same *Archiv*, p. 255, H. Zuppinger relates a method of demonstrating the Axis-cylinder Processes of the ganglion-cells of the spinal marrow.——F. Darwin contributes to *Quart. Journ. Mic. Science*, April, 1874, a paper on the Sympathetic Ganglia of the Bladder in their relation to the vascular system. The course of the nerves corresponds in a general way with that of the chief blood-vessels. Branches from the ganglia could be traced to the arteries; veins appear to be very scantily supplied with nerves. The author distinctly saw delicate nerve-fibres arise from the cells of a ganglion and supply the neighbouring capillaries, which in some cases formed part of the vascular plexus which surrounds the ganglion.——Moritz Benedikt communicates (*Virchow's Archiv*, lix. 395) some observations on the Innervation of the Inferior Choroid-plexus.——On p. 511 of the same *Archiv*, R. Arndt enquires into the Pathological Anatomy of the Central Organs of the Nervous System.

Electrical Organs.—F. Boll in *Schultze's Archiv*, x. p. 208, gives an account of the structure of the electrical plates of *Malapterurus*, and on p. 101 of *Torpedo*.

Kidney.—R. Heidenhain in *Schultze's Archiv*, x. p. 1, contributes to the Anatomy and Physiology of the Kidney.

OVARY.—W. Romiti makes some observations on the structure and development of the ovary and Wolffian duct, *Schultze's Archiv*, x. p. 200. He confirms Waldeyer's statement that the ovary possesses a cellular investment different from the endothelial cells of the peritoneum.

PLACENTA.—J. Mauthner communicates a short paper (*Sitzb. Akad. der Wiss.*, Vienna, 1873, p. 118) on the *Maternal Circulation in the Rabbit's Placenta*, with reference especially to its relations to the human placenta. When a section is made through the placenta whilst still attached to the wall of the uterus, a uniformly thick clear layer, which consists of a very fine network with interspersed nuclei, is seen next the uterus. Then follows the cellular part of the placenta, commonly named the *pl. uterina*, but which Winkler has named *basal-platte* (*Report*, VIII. p. 163); this layer is much thicker than in man, and consists of an indistinct, granular-looking conglomerate of cell-elements, which possess distinctness only where the maternal vessels pierce the *basal-platte.* Then succeeds the so-named fœtal part of the placenta, which is subdivided into many small lobules, separated from each other by furrows. Each lobule contains a strong-walled maternal artery and comparatively thin-walled veins, so that each such lobule for itself alone corresponds to the entire human placenta, because, in both, arteries and veins are separated from the fœtal villi by intermediate blood-spaces: but these blood-spaces differ fundamentally from the human, in that they are in the rabbit fine capillaries, while in man they are widely expanded spaces. In both, however, the maternal blood-channels have no proper walls, but are bounded directly by the epithelium of the villus. The villi are so adapted to each other that the epithelial investments of adjacent villous folds become blended with each other, and the maternal blood-spaces lie between the two rows of investing epithelial cells.——M. X. Delore in *Gaz. Médicale*, 21 Feb. 1874, confirms the "demonstration by Weber, Kölliker, Turner and Winkler of the circulation of the maternal blood through the *human placenta.*" He finds an epithelium in the circular sinus, but has not seen it on the placental villi.——G. B. Ercolani communicates (*Mem. dell' Accad. delle Scienze di Bologna*, IV. 1874) a memoir on the *Structure of the Caduca Uterina* in two cases of extra-uterine pregnancy. He arrives at the following conclusions: the whole extent of the uterine decidua has the power of forming a placenta, but its actual place of formation is determined by the position which the ovum takes up; there the development of the placenta goes on, whilst elsewhere its development is arrested. There is no difference in the decidua in its first stage of development between a case of normal pregnancy and an extra-uterine pregnancy; but while in the normal decidua development ceases except in the position of the ovum, in the extra-uterine it advances further, for a rich vascularity appears amongst the cellular elements, just as in the serotina, and a partial ectasy of some of the vessels, indicating the beginning of the formation of lacunæ, takes place. Whilst the new formation in normal cases only invades the

subjacent uterine tissue in the region of the ovum, in extra-uterine the whole of the subjacent uterine tissue is invaded. He does not believe in the existence of a mucosa in the human uterus, as he finds only a fine membrane on which the epithelium rests. As the decidual formation in these extra-uterine cases extended to the musculo-glandular layer, it could not be formed exclusively of the uterine epithelium, nor, owing to the absence of a true mucosa, from a sub-epithelial connective tissue. From the multitudes of new cells, having the appearance of white blood-corpuscles, he believes them to be derived from the blood by migration, and he associates with this the well-known fact, that white blood-corpuscles abound in the blood of pregnant women. The utricular glands are altered in the decidua in extra-uterine cases; in one case at two months large spaces, lined by layers of conical epithelium, were seen near the muscular layer, and in the layer of decidua, superficial to these spaces, traces of compressed gland-tubes were seen. The new-formed elements of the decidua compress the gland-tubes, and the secretion, being prevented from escaping, dilates the terminal branches, and thus the muscular bundles are broken up or pushed aside so as to present a trabecular arrangement. Amidst the muscular elements of the trabeculæ there was an enormous infiltration of the white corpuscles, more especially in the second case at the fifth month of pregnancy, and a similar infiltration amidst the cells of the serotina. As from the want of the ovum the fœtal part of the placenta cannot be completed, the dilated vessels of the decidua become stopped up by the formation of clots in their interior.

REPORT ON PHYSIOLOGY. By WILLIAM STIRLING, D.Sc., M.B., C.M. (Edinb.), *formerly Demonstrator of Practical Natural History in the University of Edinburgh*[1].

Nervous System.

FUNCTIONS OF THE BRAIN.—Nothnagel (*Virchow's Archiv*, LVIII. 420) has continued his researches on this subject. He experimented exclusively on the brain of the rabbit. On puncturing with a fine microscopic needle a spot lying in the interior of the posterior end of the cerebrum, the animal sprang from the table and exhibited unusually violent spasmodic movements, which appeared either at the time of puncture or a second or so thereafter (at the latest two minutes), and lasted not longer than three minutes. No alteration of the sensibility of any part of the body was to be observed. The author for the present refrains from suggesting any hypothesis as to the probable cause of these movements. As to the Cornu Ammonis, on puncturing it with a fine microscopical needle no disturbance in any direction was observed. Many of the animals so injured had meningitis with dyspnœa, but dyspnœa and death without meningitis was also observed. [These results are at variance with those of Fournier obtained by the injection-method, *Journ. of Anat. and Phys.* VIII. 179.] More than forty experiments in different ways and different directions were made on the thalamus opticus. Slight disturbances of the superficial layers were without effect. In a few cases the paralysis of the extensors of the finger observed by Schiff occurred. If punctured more deeply and towards the middle line, the limbs of the opposite side were directed towards the middle line. This was specially and almost exclusively observed of the fore-limbs. The deviation was the more pronounced, the more basal the direction of the puncture. The deviation in all cases was only temporary, disappearing sometimes after a few hours, in most cases after twenty-four hours. In other cases, immediately after puncture, the head was turned to the opposite side, the fore-limbs strongly divergent, the one directed outwards, the other (opposite side from injury) towards the middle line. No disturbance of sensibility. The phenomena persisted, but with decreasing intensity. After death the seat of injury was found in the posterior half of the thalamus, unusually deep towards its base, and near to the region of the pedunculus cerebri. Horizontal-section of the thalamus from above downwards was followed by the phenomena already described by Schiff (*Lehrbuch der Muskel- und Nervenphysiologie*). No change of sensibility was observed. Sometimes it appeared that just after the operation a slight hyperalgesia specially corresponding to the wounded side occurred, but it was not very convincing. This much

[1] To assist in rendering this report more complete, authors are invited to send copies of their papers to Dr Stirling, Edinburgh University.

is certain, that on division of the thalamus, no anæsthesia of the anterior extremities was produced.——"Extirpation of both nuclei lenticulares." Nothnagel, *Centralblatt*, No. 56, 1873.——"EXAMEN DE QUELQUES POINTS DE LA PHYSIOLOGIE DU CERVEAU," par le Dr Eugène Dupuy, Paris, Ad. Delahaye, 1873. Dupuy has arrived at results different from those of Ferrier. The conclusions he draws from his experiments are: (1) It is possible, by irritating certain limited points of the cortical layer of the brain, to produce contractions, sometimes of an entire limb. (2) Generally it is the fore-limb, and of the opposite side to that of the point of irritation, which is the seat of contraction. (3) The electrical current must be propagated to the base of the brain in order to excite it; either to the nerves which arise on the base itself, or medulla. (4) If the dura mater be excited by electricity, we also obtain contractions in one of the fore-limbs, generally in a crossed manner. (5) The fact that a galvanoscopic frog has been thrown into a state of contraction when its nerve touched a part of the cerebral mass, far from the point of excitation, confirms the idea that the electric current is propagated. (6) Contrary to the results obtained by Ferrier, we have never succeeded in obtaining the effects upon the tongue, whether of projection or of retraction. (7) The whole of the cortical layer of the brain is probably a centre of reflexion of a certain kind of sensibility, capable of acting in a reflex manner on motor or sensitive centres, but its integrity is not indispensable to the manifestation of voluntary and even intelligent actions. (8) In the case of the animals (dogs) on which he experimented, it was possible to excite contractions of the muscles of the entire limbs on the opposite side of the body, even after the removal of the opto-striated bodies of the opposite side. (Also *Lond. Med. Rec.* Vol. II. No. 56.)——Carville and Duret, in a communication read before the *Soc. de Biologie*, enter into a criticism on the results of the experiments of Fritsch, Hitzig and Ferrier, and add a series of experiments of their own to determine whether secondary currents are caused in the brain by the application of induced electricity when used as a means of stimulating that organ. They praise very highly the injection of chloral into the veins for producing sleep in the animals to be operated on, and rank it high above chloroform for this purpose. (This method is that of M. Oré, for the results of whose experiments with this drug see the *Lond. Med. Rec.* Vol. II. No. 64.) These authors have arrived at the following conclusions: (1) Even feeble induced currents diffuse on the surface of the brain from one point to another. (2) This diffusion on the surface is caused by the fluids and by the cerebral pulp. (3) Even feeble induced currents cannot be localised to the depth of the gray cerebral matter; they diffuse more or less deeply into the subjacent white strata. It is probable that in this case they follow a certain determinate direction, perhaps that of the principal white bundles which lead to the corpora striata or the peduncles. In the *Gaz. Méd. de Paris*, No. 4, a continuation of these experiments on the excitability of the hemispheres is published. They show that the

phenomena described by Ferrier, and which these authors have also obtained, are not due to stimulation of centres placed in the cortical substance of the brain, and ascribe them to a stimulation transmitted from the surface to the cerebral ganglia and the peduncles. They find a difference in the results when the animal (dog) is only incompletely or completely narcotised. When incompletely narcotised, the movements obtained by stimulating the surface of the hemispheres with Faradic electricity are of two orders: (1) Movements due to the diffusion of the current by the liquids, movements of rotation of the head to the opposite side, quivering of the jaw, and movements of the eyelids. (2) Movements due to the diffusion of the current into the depth of the cerebral mass, and to the excitation due to the corpora striata, movements of the limbs, and of the trunk of the opposite side. On the other hand, when the anæsthesia was complete (tested by stimulating with the induced current the central end of the divided sciatic nerve) the experimenters did not obtain any of the effects of stimulation of the convolutions, whatever the intensity of the current. Neither the centres described by Ferrier, as regulators of the combined movements of the anterior extremity, nor those of the posterior extremity, nor those of the lips and eyelids, became manifest by the electric stimulation. The authors conclude from their experiments that the peripheral layer of the hemispheres is inexcitable, it is insensible and does not contain any special motor centres. The effects obtained by Faradisation, which penetrates to the corpora striata and to the peduncles, are those of direct excitation of these organs. These effects cannot be attributed to any reflex action (as maintained by Schiff and Dupuy). Complete anæsthesia, which prevents these effects, does not change the conditions of the gray matter of the hemispheres; it only acts by diminishing the excitability, in a more or less pronounced degree, of those parts of the brain already known to be excitable.—— E. Hitzig (*Reich. und Du Bois Rey. Arch.* 1873, 397) has also continued his researches on the physiology of the brain. He confirms the results of his previous experiments with Fritsch that with increasing intensity of the current on applying both electrodes to the surface of the brain of dogs, the first contraction is produced by changing the direction of the currents, when thereby the anode comes upon the proper centre. The anode is throughout more effective than the cathode. With regard to the influence of narcotism produced by ether or morphia, the author found that during the deepest ætherisation, one or other centre did not react for a short time, whilst in others irritability remained. All reaction could only be abolished for a minimum time. With morphia narcotism the reaction of the centres remained undiminished. The same is true during the condition of apnœa. The author has found a centre for the single movements of the eyes, which coincides with a part of the centre of the facialis.

Injury to the Brain with Pulmonary Hæmorrhage.—H. Nothnagel (*Centralblatt*, No. 14, 1874) finds on injuring with a

needle a certain spot on the surface of the brain of the rabbit that peculiar disturbances occur, above all, hæmorrhage in the lungs and in the tissue of the same, often so pronounced that almost the whole lung is traversed by the hæmorrhage. Brown-Séquard, as is known, has also observed this, not however from injury to the surface of the brain, but of its basilar portion. Secondly, in the same way, meningitis can be regularly produced, chiefly bilateral, very seldom on the injured side, sometimes only on the half opposite to the injured side. This meningitis, the author thinks, is not a mere accidental circumstance.

INFLUENCE OF THE BRAIN ON THE TEMPERATURE OF THE BODY.—J. Schreiber (*Pflüg. Archiv,* VIII. 576) has operated on a large number of rabbits, with a view to determine the effect of injuries to the brain on the temperature of the body. The injury was made by means of a lancet-shaped needle introduced through the skull. The temp. was measured in the rectum. From about 70 experiments the author concludes that after injury of the pons in all parts, of the pedunculi cerebri, of the cerebellum and cerebrum, increase of the body temperature occurred when the animals were protected artificially from losing warmth; that the same results followed unconditionally and constantly on injury to the limit between the medulla and the pons. For the present the author leaves the question as to whether these results are due to the presence of a moderating excito-caloric centre, as Tscheschichin supposed, or are pure vaso-motor phenomena.

FUNCTIONS OF THE LINGUAL NERVE.—Prévost (*Arch. de Phys.* V. 253 and 375) has arrived at the following conclusions: 1. Ablation of both spheno-palatine ganglia does not affect, in dogs and cats, the sense of taste, in parts supplied by the linguals. 2. After section of the chorda tympani, in dogs and cats with cut glosso-pharyngei, the taste was little modified in some cases, notably diminished in others, and completely abolished in one. Our results do not permit us to specify the rôle the chorda plays in relation to the function of taste, but we are inclined to accord to it only an accessory part. 3. Contrary to the old views of Vulpian, and coinciding with his recent researches, the author finds that the chorda carries fibres to the terminal branches of the lingual as well as to the sub-maxillary gland. After section of the chorda, in cats, dogs, rabbits and guinea-pigs, degenerated nerve-fibres were found in the terminal branches of the lingual, as well as in the mucous layer of the tongue and sub-maxillary gland. 4. The chorda has not a trophic centre in the papillæ of the tongue, and if the sub-maxillary gland acts as a trophic centre on it, this influence ought at least to be very limited. After section of the chorda in the ear, the central end of this nerve (on the side of its emergence facially) remains healthy.

INFLUENCE OF EXTIRPATION OF THE SUPERIOR CERVICAL GANGLION ON THE MOVEMENTS OF THE IRIS.—Vulpian (*Arch. de Phys.* VI. 177) removed completely this ganglion in dogs, on the left side. At the end

of from 10 to 15 days the animals were curarised and artificial respiration kept up. The skin and subjacent tissues of different parts of the body were stimulated by strong induced currents. Each time, the pupil on the left side dilated a little from one quarter to a third of its radius. It is therefore certain that all the sympathetic nerve-fibres which act on the iris do not traverse the superior cervical ganglion, nor do all of them pass through the part of the sympathetic above this ganglion, for it was excised at the same time as the ganglion.

"Vulpian on Section of the Chorda Tympani."—Abstract in *Lond. Med. Rec.* No. 46.——"Experiments relative to the physiology of the vaso-dilating nerves." Vulpian, *Arch. de Phys.* VI. 175.

INFLUENCE OF CHANGES OF TEMPERATURE ON THE CENTRAL ENDS OF THE CARDIAC NERVES.—Tarachanoff, under Cyon's direction (*Pflüger's Archiv*, VIII. 347), finds, in opposition to Fick, who says that the central ends of the cardiac nerves are not influenced by changes of temperature, that, in passing defibrinated blood under normal pressure through the vessels of the brain, after ligature of the carotid and vertebral arteries, and of the veins, sudden increase of temperature of about 18° C. acts as a stimulant in the most powerful manner, on the central ends of the vagus, and quite in accordance with the action on the peripheral ends of this nerve.

INFLUENCE OF THE POSTERIOR NERVE-ROOTS ON THE SENSIBILITY OF THE ANTERIOR.—Cyon (*Pflüger's Archiv*, VIII. 340), in opposition to the negative results of G. Heidenhain, cites the following experiment from Steinmann's paper, as demonstrating Brondgeest's tonus. The gastrocnemius of a frog is weighted with twenty or thirty grammes, and the muscle is allowed, while at rest, to write upon a rotating cylinder. The posterior roots are then cut carefully with sharp scissors, and the muscle is allowed to write its length further. The weighted muscle increases in length in a marked degree, either immediately or in the course of a minute.

ACTION OF STRYCHNIA ON SENSORY NERVES.—Busch (*Berl. Klin. Wochensch.* No. 37) finds that sensibility is so much impaired in frogs poisoned by strychnia that pinching the animals' toes or burning the central end of the divided sciatic nerve may be performed without being followed by a reflex action.——"The Physiology of Man. Vol. IV. The Nervous System." By Austin Flint, jun., M.D. 8vo. pp. 470. New York: D. Appleton & Co. 1872.——"Physiological Studies on the Motor Nerve of the Heart; Relation of Motor and Sensory Nerves to their centres of Nutrition; Relation of the Roots of the Spinal Nerves to the Sympathetic; Excitability of the Spinal Cord, etc." Abstract of these papers from the Italian by Boll, in *Centralblatt* No. 52, 1873.——"Experimental Investigation of the simplest Psychical Processes." Sig. Exner, *Pflüg. Arch.* VII. 601 (Abstract in *Lond. Med. Recd.* II. No. 55).——"Paralysis of the Vagus in Man." P. Guttmann, *Virch. Arch.* LIX. 51.——"New formation of Brain Substance in the form of a Tumour on the surface of the Convolutions."

T. Simon, *Virch. Arch.* LVIII. 310.——"Effects of a rise of Temperature on Reflex action in the Frog." M. Foster, *Journ. of Anat. and Phys.* VIII. 45; also *Nature*, Dec. 4th, 1873.——"Sensation in the Spinal Cord," by G. H. Lewes, and Reply in *Nature*, Dec. 11th, by M. Foster.——"Double Nerve Stimulation." Dew-Smith. *Ibid.* (Abstract of both papers in *Lond. Med. Recd.* Vol. II. No. 59.)

GENERAL PHYSIOLOGY OF THE NERVES.—"Electrotonus." L. Hermann, *Pflüg. Arch.* VII. 301, 323, and 497, Abstract in *Centralblatt*, No. 43, 1873.——"Transverse conduction in the Nerves of the Frog." E. Hitzig, *Pflüg. Arch.* VII. 263.——"Form of the Curve in the so-called transverse conduction in the Nerves of the Frog." W. Filehne, *Ibid.* VII. 71.——"Action of the electrical current in different directions of the nerves and muscles." Bernheim, 60, *Ibid.* ——"Nerve Degeneration and Nerve Regeneration." H. Eichhorst, *Virch. Arch.* LIX. 1.——"On the union end to end of Sensory with Motor Fibres." Vulpian in *Gaz. Méd. de Paris*, No. 7, 1874.

EYE.—"Influence of Spectacles on the acuteness of Vision." Donders, in *v. Gräfe's Arch.* XVIII. 2. 245 (Abstr. in *Centralblatt*, No. 47, 1873).——"New apparatus for measuring the field of vision." Scherk, *Zehend. Klin. Monatsbl. f. Augenheilk.* X. 1873, 151 (Abstract in *Centralblatt*, No. 40, 1873).——"Development of Traumatic Keratitis." A. Böttcher, *Dorpat. Medic. Zeitschr.* 1873, IV. 66 (Abstract in *Centralblatt*, No. 52, 1873).——"Micrometry of the posterior part of the Eye." Laqueur, *Centralblatt*, No. 59, 1873.——"Lymph Sheaths of the Choroidal Vessels." Morano, *Centralblatt*, No. 1, 1874. ——"Inflammation of the Cornea." C. J. Eberth, *Centralblatt*, No. 6, 1874.——"Change of Fluids in the Eye." Th. Leber, *Arch. f. Ophthal.* XIX. 87 (Abst. in *Centralblatt*, Nos. 9 & 10, 1874).——"On the accommodation movement of the Choroid in the Eye of Man, the Ape and the Cat." Hensen and Völkers, *Arch. f. Ophthal.* XIX. 156 (Abst. in *Centralblatt*, No. 11, 1874).——"Estimation of the Refractive Index of the Fluid Media of the Human Eye." J. Hirschberg, *Centralblatt*, No. 13, 1874.——"Action of the Galvanic Current on the Human Eye." H. Schliephake, *Pflüg. Arch.* VIII. 565.

EAR.—"The Mechanism of the Ossicles of the Ear and Membrana Tympani." By H. Helmholtz, translated from the German, by Albert H. Buck and N. Smith. New York: William Wood & Co. 1873. pp. 69.——"Mechanism of the Ear." J. G. McKendrick, *Edinr. Med. Jour.* CCXXIII. 577.——"Mechanism of Opening and Closing the Eustachian Tube." Yule in *Journ. of Anat. and Physiol.* VIII. 127.——"Effect of the Galvanic Current upon the Acoustic Nerve." C. J. Blake, *Arch. of Scient. and Prac. Med.* New York, 1873. No. 4. 326.——"The External Ear a Synthetic Resonator." C. H. Burnett, *Philad. Med. Times*, 1873, No. 101.

FUNCTIONS OF THE SEMICIRCULAR CANALS.—Solucha has made experiments on this subject under Cyon's directions (*Pflüger's Archiv*, Vol. VIII.). The results obtained by Flourens are already well known.

Goltz ascribes the disturbance of movement produced by section of these organs, to the loss of the feeling of equilibrium; while Löwenberg regards the collective disturbance of movement as only consequences of a reflex stimulation of the nerves, which run in the membranous canals. The first point investigated by Solucha was, how far is an abnormal position of the head able to disturb the feeling of equilibrium of the animal, and so to produce the abnormal movements? The author confirms the experiment of Longet that mere section of the recti capitis postici majores et minores in the dog renders the movements of the animal uncertain and insecure; the dog was unsteady on its feet, moved from side to side, kept the fore feet widely apart from each other, running was rendered difficult, &c. After five or six days the head generally assumed the normal position, and at the same time the walking became normal. In a second series of experiments the author sought to give pigeons a peculiar position of the head, without wounding important parts—a position such as occurs on section of the canals, with the beak directed upwards, and the occiput towards the ground. On fixing the head to the breast in this position with a thread, the animals conducted themselves partly like those in which the horizontal as well as the vertical semicircular canals were destroyed. They could not retain their equilibrium, but moved to and fro on their legs, and always sought for a third point of support, made movements *de manége*, &c. As soon as the head was freed, the whole disturbance of movement disappeared, and locomotion became normal. This experiment also shows the importance of a normal position of the head, so that the animal may be able to preserve its position of equilibrium, as well as to execute co-ordinated movements. In what way is the feeling of equilibrium dependent on a normal position of the head? The answer to this is, for the greatest part, to be sought in the changes which our conceptions of the position and distance of outer objects bear in relation to our own body. In pigeons, provided with spectacles with prismatic glasses, which produced artificial strabismus, disturbance of movement distinctly analogous to the higher degrees of that which occurs after section of the semicircular canals was observed. In some cases even 'pendulum movements,' corresponding to those seen on section of the horizontal canals, were noted. On section of one horizontal canal, the animal made several lateral movements of the head, beginning from the injured side, which soon ceased. On section of the corresponding canal on the opposite side 'pendulum movements' of the head occurred, and persisted very long. The violence of the movements increased from the beginning onwards, until they reached a maximum, when the animal lost its equilibrium, fell over, executed movements *de manége*, &c. In a few cases the animals recovered completely, but generally after four or five days the animal was found in a corner with the peculiar position of the head above described, and quite quiet, but when disturbed it resumed the pendulum movements, &c. Most of the animals died in from ten to twenty days. On section, the neighbouring parts of the skull were bloody and infiltrated, and the cerebellum was softened on the posterior surface and of a yellow-

ish-green colour. The results of section of the smaller vertical canals were in some respects similar to the above, but differed in some important points. In section of the horizontal canals, the head moved in a horizontal plane from right to left, and back again; but in animals with cut vertical canals, the pendulum movements of the head occurred from above downwards and back again—then in a vertical plane. The subsequent movements of the trunk were also different, in that the whole trunk tumbled round on its transverse axis, and that mostly from before backwards. On section of all four canals, violent movement of the head, resembling a screw motion, occurred immediately, accompanied by general swinging movement of the whole body. That disturbance of equilibrium is a direct consequence of section of the semicircular canals is certain. 1. These disturbances occur immediately after the operation, and this when it is free from all other complications, as section of muscles, bleeding, injury to the cerebellum, &c. 2. The two sorts of movement, as well of the head as of the trunk, scarcely admit of a doubt, that the semicircular canals stand in relation to certain conceptions of space, and sensation. By means of the nerves which end in the membranous canals, a series of unconscious impressions are continually communicated, which lead to unconscious conclusions as to the position of the head in space. The semicircular canals contribute only indirectly to the retention of the equilibrium of our body, in that they direct the position of the head in space. The chief results of these experiments are the following: 1. For retention of equilibrium, it is necessary that the animal has correct conceptions as to the position of its head. 2. The semicircular canals possess the functions of informing the animal, by a series of unconscious (auditory?) impressions, as to the position of his head in space, and each semicircular canal has an exact relation to a dimension of space. 3. The movements which occur after section of the semicircular canals are of three sorts: *a.* Disturbance in equilibrium, as the direct consequence of the injury; *b.* Swinging movements, as consequences of stimulations arising from abnormal auditory sensations; *c.* Consecutive phenomena, produced by inflammation of the cerebellum, occurring several days after the injury (*Lond. Med. Rec.* Vol. II. No. 58).——"On the Sense of Rotation and the Function of the Semicircular Canals." A. Crum Brown, *Proceeds. Roy. Soc. Edin.* 1873—74, and this *Journal*, 1874.

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Circulatory System.

Bouillaud "on normal and abnormal pulse." Abstract in *Lond. Med. Recd.* I. No. 42. "Action and Sounds of the Heart," G. Paton, *Edin. Med. Journ.* CCXXI. 407 (Abstract in *Lond. Med. Recd.* I. No. 48). "Blood-pressure in the heart and arteries," Fick, *Verhandl. d. physik. medic. Gesellsch. zu Würzburg*, IV. 223 (Abstract in *Lond.*

Med. Recd. No. 49).——"Quinine and Blood." Binz, *Arch. f. Exper. Pathol.* 1873, I. 18.——"Action of Atropin and Physostigma on the pupil and heart." Rossbach and Fröhlich, *Würzb. phys. med. Verh.* I. (Abstract in *Centralblatt*, Nos. 58 and 59, 1873).——"Origin of Hæmorrhages after ligature of the vessels." *Virch. Arch.* LVII. 436 (Abstract in *Centralblatt*, No. 59, 1873).——"The Capacity of blood for Oxygen at different barometric pressures." P. Bert in *Soc. de Biologie* (*Gaz. Méd. de Paris*, No. 2, 1874).——"Demonstration of the Pulse by means of the flame." R. Klemenziewiz, *Untersuch. aus d. Institut. in Graz*, Heft III.——"On Ecchymosis and other effusions of blood, caused by a nervous influence." Brown-Séquard, *Arch. of Scien. and Pract. Med.* 1873, 148.——"The red blood-corpuscles." C. Faber, *Archiv der Heilkunde*, XIV. 481 (Abs. in *Centralblatt*, No. 6, 1874).——"On Diapedesis." J. Arnold, *Virch. Arch.* LVIII. 203 (Abs. in *Centralblatt*, No. 8, 1874).——"Action of Calabar Bean on the Heart." H. Köhler, *Arch. f. exp. Pathol.* 1873, I. 277 (Abs. in *Centralblatt*, No. 8, 1874).——"On the migration of white corpuscles into the lymphatics of the frog's tongue." R. Thoma (*Tageblatt d.* 46 *Versam. deutsch. Naturf. und Aerzte in Wiesbaden*, 1873 (Abs. in *Lond. Med. Recd.* I. No. 51).——"Ein Beitrag zur Mechanik der Herzcontractionen," *Inaugural Dissertation*, by E. A. Lutze, Cöthen, 1874.——"Causes of the secondary waves in the Pulse," Galabin, *Journ. of Anat. and Phys.* VIII. 1.——"The Law which regulates the frequency of the Pulse." A. H. Garrod, *Ibid.* p. 54.——"Vaso-motor and uterine Nerve-centres." Schlesinger, *Allgem. Wiener Med. Zeitung*, Nos. 42 and 43 (Abstract in *Lond. Med. Recd.* II. No. 55).——"The rôle of Oxygen in the formation of Pus." C. Binz, *Virch. Arch.* LIX. 293.——"The proportion of the white to the red Corpuscles after Suppuration." Apolant, *Ibid.* LIX. 299.——"Doctrine of the reflex stimulation of the vaso-motor nerves." Cyon, *Pflüg. Arch.* VIII. 327.——"The resistance of the walls of the vessels in the normal condition and during Inflammation." F. v. Winiwarter, *Sitz. d. k. Akad. d. Wissensch.* LXVIII. 3 Abth. 1873.——"Direct electrical stimulation of the Mammalian Heart." S. Mayer, *Ibid.*

"Observations on Welcker's method of Estimating the Quantity of Blood."—R. Gscheilden, *Pflüg. Arch.* VII. 530, disposes of objections which had been urged by Ranke against the method of estimating the quantity of blood in the body, employed by Heidenhain and himself, for which see the original.

I. Brozeit suggested that the quantity of blood would be found to be smaller when the animal had been previously subjected to an important injury. The author, to determine this, experimented upon rabbits. From the animal first a small quantity of blood was withdrawn and its amount of solid constituents determined. It was then subjected to various operations, and then a second quantity of blood withdrawn and its solid constituents likewise determined. The operations that the rabbits were subjected to were, throwing the whole animal into a state of tetanus by the application of induction shocks to the spinal cord; poisoning with strychnia; section of the spinal

cord and artificial respiration; suffocation, by the mechanical cessation of the respiration, by CO, and common gas. In all cases the blood showed a slight diminution in solid constituents, but this was also found in the second blood-test when the animal was left uninjured, and was the greater, the smaller the animal employed.

II. The quantity of blood in rabbits, guinea-pigs, and dogs. Ranke gives great variations in rabbits, from 1 : 12,25 to 1 : 34,4. In two new experiments the author found it in comparison to the nett weight of the body (weight of animal minus intestines) as 1 : 19,2 and 1 : 18,2. His former result was 1 : 19,5.

III. Colouring matter of the muscles of different animals. The author shows that Brozeit's view as to the colour of muscle being due to diffusion of hæmoglobin is quite untenable. Whether the quantity of the colouring matter in muscle depends upon the activity of the muscle the author's experiments do not definitely determine.

Periodicity of the Action of the Heart.—Regarding the experiments of Metschnikoff and Setschenow (*Journ. of Anat. and Phys.* viii. 193), A. B. Meyer adds a note in *Centralblatt*, No. 59, 1873, to say that he has observed the phenomena described by these authors and announced it under the term "Intermittenz," in his paper "*Ueber das Hemmingsnervensystem des Herzens.*"

Chemical composition of the Blood-corpuscles.—J. Paquelin and Jolly, in a memoir presented to the *Soc. de Biologie* (*Gaz. méd. de Paris*, No. 7, 1874), sum up the results of their researches on this subject thus: 1. We do not know any process which yields hæmatosine in a state of purity. 2. This substance is always more or less mixed with albuminous matter. 3. The albuminous matter varies in quantity according to the degree of alkalinity of the solvents employed. 4. Other substances, such as iron, may be mixed with hæmatosine according to the mode of preparation. 5. The differences of opinion regarding the constitution of hæmatosine are a consequence of the difficulties inherent to its preparation. 6. Hæmatosine contains no iron. 7. The ferruginous principle of the corpuscles is combined with an albuminous body soluble in heat, alcoholic fluids, but strongly in alkaline ones. 8. Incineration changes the constitution of the mineral principles of the corpuscles. 9. Carbonization does not alter these principles. 10. Iron exists in the corpuscles in the state of phosphate of the protoxide. 11. This phosphate of iron is tribasic. 12. All the phosphates of the organism may be represented by the formula 3 $(MO)_1$ PO_5. 13. The basicity of these phosphates is less stable. 14. 100 grms. of dried corpuscles yield

Phosphoric acid	0,415
Lime	0,015
Protoxide of Iron	0,600
Potass	0,031
	1,061

which corresponds to

Phosphate	of Potass	0,046
„	of Lime	0,027
„	of Iron	0,994
		1,067

The method of (1) extracting the corpuscles, (2) separating the hæmatosine, (3) analysing the mineral substances, is detailed in the original. The experiments were made on the blood of the ox.

Origin of Mechanical Œdema.—A. Hehn (*Centralblatt*, No. 40, 1873) has repeated Ranvier's experiments on this subject and arrived at the following conclusions. The author experimented on dogs. 1. Extraperitoneal ligature of the vena cava inferior below the renal veins was followed, in a large number of animals, by neither œdema of the lower extremities nor collection of transudation in the peritoneal cavity. 2. Ligature of the vena jugularis on both sides yielded also negative results. 3. Section of one sciatic nerve with simultaneous ligature of the vena cava inferior produces œdema in the corresponding limb only when the nerve is divided at a certain height after its exit from the ischiadic foramen.

Action of Zinc on Blood-Solution.—H. Struve (*Journ. of Pract. Chem.* N. F. vii. 1873, 346). Schönbein has shown that when Zn is shaken with water a small quantity of hydric peroxide is formed. The author finds that simple contact of Zn with water is sufficient for this. Place Zn in a solution of blood in a similar way and allow it to stand, gradually a brownish red precipitate is formed and the fluid becomes less and less coloured, and finally becomes as clear as water. The precipitate contains the whole colouring matter of the blood, and the filtrate is free from albumen.

Transfusion of Blood.—L. Landois (*Centralblatt*, Nos. 56 and 57, 1873) injected into dogs the blood of the sheep, cat, guinea-pig, rabbit, man, pig, calf, and pigeon. The blood of the hare, sheep, calf, and man was transfused into the rabbit. In a special series of experiments the frog was employed, and into it blood from the frog, rabbit, sheep, calf, guinea-pig, pigeon, pike, and man, was injected. With regard to the experiments upon the mammalia, the author remarks—A. That the blood-serum of many mammals dissolves the blood-corpuscles of other mammals. The most active serum (as yet examined) is that of the dog, the least active is rabbit-serum. B. The blood-corpuscles of mammals possess quite another capability of resistance in the serum of other animals, e.g. the blood-corpuscles of the rabbit are exceedingly easily dissolved, whilst those of the dog and cat manifest great resistance. The manner of solution can be observed under the microscope. At blood-heat the solution takes place quicker than at a lower temperature. Amongst others the author draws the following conclusions from his experiments. The dissolved constituents of the blood-corpuscles are excreted principally by the urine, less

richly and not so constantly by the intestine, uterus, bronchial tubes, and into the serous cavities. A certain quantity of the dissolved materials can be used for the construction of the body of the recipient. The beginning and the end of the excretion of blood by the urine varies. Already after $1\frac{3}{4}$, $2\frac{1}{2}$ hours after the injection hæmoglobin and albumen were found in the urine. The same condition existed even twelve hours thereafter and also later. The quantity and kind of blood transfused and the condition of the vascular system is in this respect of influence. When foreign blood is transfused into an animal, some of the animal's own blood-corpuscles may disintegrate and dissolve. This is the case when the blood-corpuscles of the recipient are soluble in the serum of the animal which yields the blood. In animals with easily soluble blood-corpuscles, e.g. the rabbit, the injection of serum of different sorts, e.g. of the dog, man, pig, sheep, cat, produces, according to the quantity, very menacing symptoms; increase of the frequency of respiration, dyspnœa, convulsions, even death or asphyxia: whilst animals with resisting blood-corpuscles, e.g. the dog, can bear the injection of other kinds of serum, e.g. of the sheep, ox, pig, without any of these phenomena. After too great a transfusion with solution of the blood-corpuscles the formation of clots of fibrin sometimes occurs, causing the death of the animal.

ACTION OF BITTER SUBSTANCES ON THE CIRCULATION AND BLOOD-PRESSURE.—Köhler (*Tageblatt d. 46 Versam. deutsch. Naturf. und Aerzte in Wiesbaden*, 1873) finds that Cetrarin, Columbin, and probably the other bitter substances, when injected into the veins produced a diminution of the arterial blood-pressure (8—20 Millim. Hg.) followed by a gradual increase of 12—18 Millim. Hg. above the original tension. The cause of the diminution of pressure, which occurred even after section of spinal cord and vagus, is to be sought in the heart itself; whilst the cause of the increase, which did not take place after section of the spinal cord, must be sought for outside the heart. The increase of the blood-pressure after injection of cetrarin and columbin is to be ascribed to the excitation of the vaso-motor centre. The initial diminution was also observed after paralysis of the termination of the vagus in the heart. The frequency of contractions of the heart, even when poisonous doses of the above substances had been given, remained unchanged till shortly before death.

A PECULIARITY OF CAPILLARY-BLOOD.—F. Falk (*Virch. Arch.* LIX. 26) asks why it is that blood is found fluid in the capillaries post-mortem, and why even after removal from the body it remains in the same condition. He set himself to determine which of the three substances necessary for the coagulation of the blood [(fibrino-plastic substance, fibrinogen, and the coagulating ferment (Schmidt)] is absent. The blood was obtained by puncture from the human lungs or from those of the horse. The presence of fibrino-plastic substance was proved by diluting the blood with twenty parts of water and care-

fully adding acetic acid, or leading through it a strong stream of CO_2, when a precipitate showing the properties of paraglobulin was obtained. This precipitate placed in the fluid of a hydrocele or in pericardial fluid yielded fibrin. On treating the blood after the manner recommended by Schmidt for obtaining the fibrin-ferment, a fluid was obtained which accelerated the coagulation, but not to an extent equal to that obtained from normal blood or serum. No coagulation occurred however on adding to the capillary post-mortem blood, defibrinated blood (which contains fibrin-plastic substance). This points to the absence of the fibrinogen. In fact the author could not obtain this substance by any of the means recommended for its separation. The non-coagulability therefore of capillary blood from the dead body is due to the absence or considerable diminution of the fibrinogen.

INFLUENCE OF NUTRITION ON CARDIAC MUSCLES.—L. Perl (*Virch. Arch.* LIX. 39, Abst. in *Centralblatt*, No. 15, 1874), in his first series of experiments (on dogs), performed seldom and large bleedings (every 5—7 days, 3—3½ per cent. of the body weight at each time), in a second series more frequent and smaller bleedings (every 3—4 days, 1—1½ per cent.) were practised. The animals endured the operative procedures well. The wounds healed well without fever, and only in one case did embolus of the lungs occur. Whilst the animals of the second series, on which ten as the minimum, and seventeen as the maximum, of bleedings were practised, remained quite cheerful and well, and when killed, from the 36—39th day, showed no signs of change in the muscles of the heart, the seven dogs of the first series, on the contrary, on which 5—7 bleedings were practised, became lean, lost appetite, became sad, had partial œdema of the extremities, and died (6) with the phenomena of marasmus within eleven weeks. With a single exception, all the animals dying after four weeks showed a very flabby heart, with a yellowish colour, and under the microscope a large number of the muscular fibres were found to have undergone extensive fatty degeneration. These fibres lay irregularly scattered amongst normal ones, for the most part in the papillary muscles, especially of the left half.

REFLEX INNERVATION OF VESSELS.—E. Pick (*Inaug. Dissert.* Berlin 1873, 8°, 30 p., and *Reichert und du Bois' Archiv*, 1873, I.) experimented on the web of the second interdigital space of Rana temporaria. The main artery and vein run close to the bone, and give off a series of branches which divide dichotomously until they end in the capillaries. Dilute acetic acid or the induced current were used as the stimulating agents. The greater the stimulation, and the smaller the vessel, the quicker and more intensive is the reflex contraction. Different portions of skin require different intensities of the stimulant to produce the same effect. The most sensitive part is the skin of the back, the least that of the face. Intense irritation of the larger arteries is followed by dilatation. The vaso-motor channels run exclusively in the ischiadicus.

NUMBER OF RED AND WHITE CORPUSCLES IN THE BLOOD.—Melassez (*De la Numération des Globules Rouges du Sang*, Paris, 1873, and *Arch. de Phys.* VI. 32) describes his method of estimating the number of corpuscles, red and white, in the blood. Amongst others he has arrived at the following conclusions.

Arterial blood seems to have the same richness in corpuscles in the great trunks; in an arteriole he has found the corpuscles increased in number. Venous blood is very different according to its source and to the state of activity of function of the organs from which it comes. In the skin, the richness in corpuscles ("la richesse globulaire") increases. This augmentation is less marked on suppression of the cutaneous evaporation and when congestion is produced either by paralysis or by stimulation of the sympathetic; it is, on the contrary, more considerable when the cutaneous evaporation is increased, or when congestion is produced by an obstacle to the circulation. A similar augmentation is observed in muscles. It is least when the muscle is at rest or paralysed, and is much increased when the muscle contracts. In glands, the increase is more considerable during the state of repose, than when the gland is in action. The number of red corpuscles is increased in the spleen, and most so during the period of digestion. In the liver, their number appeared to the author to be diminished. Then follows an estimation of the number of the red blood-corpuscles in the mammalia (*Comptes Rend.* Dec. 1872 and Abs. in *Lond. Med. Rec.* No. 1, Vol. I., 1873). The author also estimates the richness of the blood in white corpuscles in cases of erysipelas and in some cases of suppuration. The author finds by his method that in a cubic millimeter of the arterial blood of the dog there are from 3,410,000 to 3,780,000 red corpuscles; in the arterial blood of the rabbit 4,700,000, and in the venous 4,900,000; in the arterial blood of the guinea-pig 3,900,000, and in the venous 4,300,000.

ACTION OF GASES IN THE COAGULATION OF ALBUMEN.—E. Mathieu and V. Urbain (*Comptes Rendus*, 1873, LXXVII. 706, Abs. in *Centralblatt*, No. 58, 1873) have found that blood-serum, when deprived of its gases, does not coagulate at 100°, and the same is true of the albumen of eggs. The gas obtained from the albumen of the egg is for the most part CO_2. If the solution of albumen is again saturated with CO_2, then heat will coagulate it. When the albuminous coagulum is treated with an acid, e. g. tartaric, CO_2 can be obtained, about 60—80 ccm. from 100 ccm. of albumen. The authors regard the coagulation occurring under heat as a combination of the albumen with the CO_2 present in the fluid. In an albuminous solution freed from CO_2, alcohol, acids and the metallic salts cause coagulation. If the pumping out of the gases is continued long enough, not only the dissolved gases are obtained, but also traces of carbonate of ammonia, as well as sulphide of ammonium, and then the solution exhibits the properties of globulin, which, according to the authors, can be precipitated in the cold by CO_2. The air-pump is not necessary to cause this change of albumen into globulin! it is sufficient to

dilute freely the solution of albumen and place it under a bell-jar with sulphuric acid and caustic potash. The addition of a small quantity of phosphate of soda gives the globulin solution the properties of cœsin. Coagulated albumen, and the different albuminous substances dissolved in NH_3, and evaporated, yield a solution, exhibiting the properties of globulin, and can be compared with Mulder's Protein.

Respiratory System.

"Action of protoxide of N. on germination and respiration; on the properties of chloropyll." Jolyet and Blanche, *Gaz. Hebdom.* 1873, p. 389.—"The respiratory centre," Gierke, *Pflüg. Arch.* VIII. 583 (Abstr. in *Centralblatt*, No. 59, 1873).—"The respiratory movements" (A physiological and pathological study), F. Riegel, Würzburg, 1873, pp. 176, 12 plates. Abs. in *Centralblatt*, No. 5, 1874.

INFLUENCE OF ARTIFICIAL RESPIRATION OVER POISONING WITH STRYCHNIA. Jochelsohn (*Verhandl. d. Physikal. Medicin. Gesellsch. zu Würzburg*, New Series, v. 107) finds in support of the experiments of Rossbach (*Jour. of Anat. and Phys.* VIII. 203), and in opposition to those of Leube and Brown-Séquard, that artificial respiration has no distinct effect upon the duration and intensity of the convulsions; and these occurred even when the animal had been rendered apnœic before the administration of the drug. The author does not believe that the strychnia is excreted or destroyed in the lungs, and thinks that artificial respiration prolongs the life of the animal poisoned by strychnia in the same way as that of animals whose spinal cord has been divided high up in the neck (fuller report in *Lond. Med. Rec.* No. 52).

ON APNŒA AND THE ACTION OF AN ENERGETIC STREAM OF CO_2 GAS ON THE MUCOUS MEMBRANE OF THE RESPIRATORY APPARATUS, &c. The results of Brown-Séquard as to the effects of a strong stream of CO_2 in cutting short an epileptic attack in guinea-pigs whose sciatic nerve or lateral half of the spinal cord has been divided, have been already noticed (*Journ. Anat. and Physiol.* VII. 343). W. Filehne (*Reich. und Du Bois Reym. Arch.* 1873, 361) has repeated the experiments of Brown-Séquard, and has always arrived at only negative results. He finds further that powerful stimulation of the soles of the feet with induced current is without effect on the epileptic attacks in guinea-pigs.

Alimentation.

"On the Separation of Digestive Ferments." V. Paschutin, *Reich. und Du Bois Reym. Arch.* 1873, 382.——"Amylotic Ferment of the Pancreas." A. Liversidge, *Journ. of Anat. and Phys.* VIII. 23.——"Chemical Properties and Physiological Action of Fat." J. Day, *Australian Med. Journ.* (abstract in *Lond. Med. Journ.* Vol. II. No. 62.——"Contributions to the Question of Nutrition." J. Forster, *Zeitsch. f. Biol.* IX. Heft 3.——"On the Butyric Acid Fermentation."

V. Paschutin, *Pflüg. Arch.* VIII. 352 (abstract in *Centralblatt*, No. 9, 1847).——"Intervention of Electro-Capillary Forces in the Production of the Phenomena of Animal and Vegetable Life." Becquerel, *Robin's Journ. de l'Anat.* Jan. 1874.——"Physiology of Defecation." Gobrecht in the *Clinic* of March, 1873 (abst. in *Lond. Med. Rec.* No. 49).——"Physiological Action of Alcohol." Sée, Translation in the *Lond. Med. Rec.* No. 50.——"Absorption of Fat in the Small Intestine." S. Thauhoffer, *Pest. Medizin. Chirurg. Presse*, 1873, No. 22 (abstract in *Centralblatt*, No. 44, 1873, and *Pflüg. Arch.* VIII.).——"Withdrawal of Alkali from the Living Body." E. Salkowski, *Virch. Arch.* 1873, LVIII. 1 (abstract in *Centralblatt*, No. 49, 1873).——"Mineral Inanition and Influence of Phosphate of Lime on the Transformation of Albuminous Substances." Brothers Dusart in *Société de Biologie* (*Gaz. Méd. de Paris*, No. 5, 1874). ——"On the Cataphoric Changes of Moist Porous Bodies." H. Munk in *Reich. und Du Bois Rey. Arch.* 1873, 241.——"Preparation of Solutions of Albumen free from Salts by means of Diffusion." R. Aronstein, and observations on the same by A. Schmidt. *Pflüg. Arch.* VIII. 75 and 93 (abstract in *Centralblatt*, No. 1, 1874).—— "On the Origin of Sulphuric Acid and the Condition of Taurin in the Animal Organism." E. Salkowski, *Virch. Arch.* LVIII., and "Synthesis of Taurocarbaminacid," *Berich. d. Deutsch. Chem. Ges. zu Berlin*, VI. 1191.——"The History of Uramid Acid." H. Huppert, *Ibid.* (abstract of these three papers in *Centralblatt*, No. 11, 1874). ——"On the Albuminous Bodies." O. Nasse, *Pflüg. Arch.* VIII. 381 (abstract in *Centralblatt*, No. 13, 1874).——"Estimation of N. in Albuminous Bodies by means of Soda Lime." Ritthausen, *Journ. f. Pract. Chem.* N. F. VIII. 10.

SECRETION OF THE PANCREAS.—L. Landau (*Inaug. Dissert.* Breslau, 1873, pp. 34, abstract in *Centralblatt*, No. 56, 1873). A graduated canula was introduced into one of the pancreatic ducts, whilst the other was ligatured along with the ductus choledochus. Heidenhain's method of using a poison to paralyse the secretory nerves was employed. The dogs were curarised and artificial respiration kept up. The mean quantity of secretion, in seven experiments, was 0,2 ccm. in 60 mins. (The results of Bernstein's experiments gave about thrice as much.) Injection of Atropin, Calabar bean, Nicotin, had no constant effect on the secretion, nor had increase or diminution of the blood-pressure any material influence upon it. Stimulation of sensory nerves, as well as electrical stimulation of the lingual, yielded only negative results. In opposition to Bernstein, who employed a permanent fistula, the author found only seldom a diminution of the secretion, after stimulation of the vagus. Often section of the vagus was without any effect, sometimes however this (along with general clonic spasms of the body) as well as its stimulation was followed by increase of the secretion. Positive results were first obtained on stimulating the medulla, either directly or by suspending the respiration. Stimulation of the medulla with induced electricity increased the pancreatic secretion, and this the more,

the oftener the irritation. Often the rapidity of outflow appeared first after cessation of the irritation.

Estimation of the N. in Albumen.—M. Maerker (after the researches of O. Abesser), *Pflüg. Arch.* VIII. 195. A large number of analyses were made to determine whether the mean percentage of N. in flesh is to be trusted. It is found that the Will-Varrentrapp method gives results very nearly accurate. The different results of Nowak and Seegen are to be referred to differences in the conduction of the analyses.

Significance of the Ash-Constituents in Food.—J. Forster (*Zeitschrift für Biologie*, Vol. IX.) has selected pigeons and dogs for his experiments on this subject. These animals were fed with food containing as few salts as possible, 'with albumen, in the form of the residue of flesh after the preparation of the "extract of meat."' This residue, as is known, is not quite free from salts, but these were extracted from it, as far as possible, by repeated boiling and washing in distilled water. By this process a powder, containing in the dried state 14·445 per cent. of nitrogen was obtained; 100 grammes of this dried substance contained—

Phosphoric acid anhydride	0·548	gramme.
Chalk (calcium oxide)	0·078	„
Iron	0·023	„
Potassium	0·151	„
	0·800	

Magnesia and chlorine were found in too small quantities to be weighed. Casein from ordinary milk, boiled in distilled water, was also employed. The starch used was treated several times with a 0·08 per cent. solution of hydrochloric acid, and washed in a filter with distilled water until the filtrate yielded no precipitate on the addition of nitric acid. A mixture of one part of casein and seven parts of starch, as used in the experiments on the pigeons, contained 0·279 grammes of phosphoric acid in 100 grammes of the dried mixture. Fat, in the form of the best butter, formed part of the food, and in addition distilled water was freely given.

A prepared mixture of the above was given to pigeons and dogs, and when it was rejected they were fed artificially. The author concludes from his experiments that the addition of certain salts is necessary for the retention of the balance of materials (*Stoffgleichgewicht*) in the animal economy. When this supply sinks below a certain limit or is completely withdrawn, the body excretes salts, and the animal dies.

The following table shows the quantity of nitrogen taken in in the food, and that excreted, in a dog. The duration of the experiment is divided into three periods of eight days each.

This table clearly shows that the removal of salts from the food has no effect on the transformation of albumen, but that this chiefly

depends upon the quantity and kind of supply of the combustible alimentary materials.

Taken in				Nitrogen Excreted			Nitrogen Difference
Flesh	Fat	Starch	Nitrogen	Urine	Fæces	Total	
I. 1,438	1,200	300	207·0	197·5	7·5	205·2	+ 1·8
II. 1,311	650	—	189·3	188·2	15·0	203·2	− 13·9
III. 1,249	689	663	180·4	182·1	16·0	198·1	− 17·7

Apart from the contents in salts, the solid as well as the fluid products excreted, during deprivation from salts, are the same as in normal nutrition. At the beginning of the experiments, the digestive juices secreted had the normal constitution, but they gradually changed. A time then arrived when they became inactive, or no longer had the normal composition.

In all animals fed with food from which the salts had as far as possible been extracted, a condition of weakness of the muscles and tremblings occurred, best characterised as general exhaustion. The weakness of the posterior limbs of the dogs assumed in the second week of the experiments a paralytic character. The activity of the brain was disturbed, as shown by the stupid appearance of the animals, etc. Phenomena of increased sensibility showed themselves later. By the greatest possible removal of the mineral constituents from the food of adult animals, the process of the changes of materials and decomposition in the body proceed in the same way, till the death of the animal, as by a diet which, in addition to the above necessary constituents, also contains the ash constituents. Latterly, however, disturbances in the functions of the organs occur, which hinder on the one side the transformation of the nutritive material into modifications capable of being absorbed, and thereby prevent the reparation of the decomposed material of the body, and on the other, by suppression of processes necessary to life, bring about the destruction of the organism, before the impossibility of a continued reception of food is followed by decline and death.

With regard to the salts excreted, when these are removed from the food, the tables show that the excretion of phosphoric acid is never interrupted. In animals deprived of salts the excretion of this substance by the urine is largely diminished. The less the quantity of food poor in salts introduced, the greater is the loss in phosphoric acid which the body suffers. The smallest quantity excreted corresponded to the time when the greatest quantity of combustible material was introduced.

Although the food was quite free from chlorine, and the urine in the latter period of the experiment contained only a trace of this substance, still the vomitings on the thirty-fourth day showed that a large quantity of chlorine was mixed in the stomach with the food introduced. 246 grammes of food vomited contained 1·63 grammes of

chlorine, while the urine excreted on the same day contained only 0·04 grammes. Chlorine was continually excreted in the stomach and absorbed again, for the fæces contained no chlorine.

The author thinks that all these conditions are to be explained by the condition of the salts in the body. He divides these into two classes. By far the greatest part is in combination with the combustible substances, chiefly with the albuminous bodies in fixed or loose combinations. A small fraction, previously in combination, but freed by decomposition and oxidation, is present in the blood, with the products of the metamorphosis of the tissues. The latter are excreted on the blood passing through the kidneys. Food free from salts, introduced into blood from the digestive canal, unites in the blood with the free salts arising from the chemical decompositions. The quantity of salts excreted must increase with the quantity of free salts in the blood. The salts excreted are increased during hunger, and this because the salts in combination with the body-substance are set free to enter the blood. An increased excretion was also observed when a surplus of salts was added to the food, as was the case at the end of one experiment.

The author is of opinion that the supply of nutritive salts, or of those salts in the food which can prevent a loss of salts from the body, is less than till now has been supposed.

Researches on Digestion and Absorption in the Human large Intestine.—V. Czerney and J. Latschenberger, *Virch. Arch.* lix. 161. (Translation in full in *Lond. Med. Rec.* Vol. ii. Nos. 62 and 63.) The authors give a full account of the literature of the subject, and then cite the history of the patient upon whom they experimented. The researches were conducted on a man with preternatural anus (of the sigmoid flexure) in the left inguinal region. The case was distinguished from the others which have been employed for physiological investigation, in the circumstance that the rectum was completely exposed by the prolapsed loop of intestine; it could be filled from above with the articles of food on which the experiments were made, and, at any desired time, emptied *per anum*. As the rectum could be washed out from above like a retort, the discharge gave at once the amount of material absorbed. While Voit and Bauer deduced from the excreta, by an indirect process, the amount of water absorbed, we were able to directly determine the quantity of unabsorbed residue.

The method of experimenting is then described. The authors have arrived at the following conclusions:—

The human rectum and its secretion has no digestive action either on coagulated albumen, or soluble albumen, or on fat.

In the normal condition, soluble albumen (dissolved in water) is absorbed from the human rectum as such, not being changed by the intestine; and a greater percentage is taken up, the longer it remains in the bowel. Any irritative condition in the bowel impedes absorption, or completely arrests it. Chloride of sodium also impedes absorption, but is itself taken up notwithstanding that the intestine is in a state

of irritation and that absorption is suspended. In the hen's egg albumen is contained in a form unfavourable for absorption.

Fat in emulsion is absorbed by the human rectum. The quantity absolutely absorbed is in proportion to the degree of concentration; and the amount per cent. absorbed is in proportion to the time during which the fluid has been in contact with the absorbing surface.

Starch in the hydrated form is absorbed by the intestine; whether as such, or after being changed into sugar, is not decided by these experiments.

The greatest quantity of albumen absorbed in 29 hours was about $1\frac{1}{2}$ grammes. As the large intestine is on an average about four times as long as the portion which served for experiment, this indicates an absorbent power, in the whole large intestine, of 6 grammes of soluble albumen from a solution containing $4\frac{1}{2}$ per cent. This quantity is far from being sufficient for nutrition, as about 120 grammes are necessary for a healthy man (Voit and Bauer). The quantity of albumen absorbed is evidently increased when more concentrated solutions are used.

General Result.—The human rectum absorbs soluble albumen unchanged, as such; it also takes up emulsion of fat; starch also is absorbed, but it remains undecided whether it be absorbed as starch, or whether it must first be changed into sugar. Chloride of sodium impedes or completely arrests absorption.

Tables are appended showing the results ~~in~~ each of the experiments.

THE MODE OF SECRETION OF GASTRIC JUICE.—H. Braun (*Eckhardt's Beiträge zur Anatomie und Physiol.* VII. I. 27. From abst. in *Centralblatt*, No. 15, 1874). The general view held regarding the secretion of the gastric juice is that it is caused by stimulants (chiefly of a mechanical and chemical nature). In opposition to this view, the author renders it probable that, just like the urine, the gastric juice is continually being secreted. He experimented on dogs with gastric fistula. Through these fistulæ the stimulating substances, small pieces of sponge, portions of feathers, sand, alkalies, and pieces of flesh, were introduced, and this without the quantity of secretion, which was obtained by means of small pieces of sponge, shewing any increase. Quite as inactive was the alkaline saliva of dogs and man. According to the author's experiments no near relation exists between the conditions of the secretion in the mouth and stomach, for neither had stimulation of the former any pronounced influence on the secretion of the gastric juice, the secretion of the gastric juice had as little effect on the secretion of the saliva. Schiff's hypothesis that pepsin is first extracted from peculiar so-called peptogeneous bodies in the stomach is rejected by the author, for completely starved animals, whose salivary ducts were cut, showed a not unimportant secretion of active gastric juice. Much more are pepsin and acids, as Spallanzani first hinted, continually secreted. The mucous membrane of the empty stomach is only seldom covered with tough mucus, often with a fluid, which has an acid reaction. Just the same as the urine, is

the gastric juice secreted in larger quantities after injection of large quantities of water into the vena femoralis. This is not a simple transudation, for the secretion has an acid reaction and digests discs of albumen normally. Often, to give the secretion digestive power, the addition of the HCl was necessary; a fact which is brought by the author in connection with the results of Manassein, that in the gastric juice of acutely anæmic animals the acids are absent. The injecting fluids employed were a 1—2 per cent. solution of urea, and 1 per cent sol. of NaCl. Whether section of one or both splanchnici increases the secretion of the gastric juice, the author, in spite of several positive results, leaves for the present undecided, for not seldom the secretion in the course of the experiment increased without appreciable cause.

FEEDING WITH FLESH AND CARBOHYDRATES AND CARBOHYDRATES ALONE.—M. Pettenkoffer and C. Voit (*Zeitschr. f. Biol.* 1873, IX. 435. From Salkowski's report in *Centralblatt*, No. 18, 1874). This paper is a continuation of their previous one on feeding with flesh and fat alone (*Journ. of Anat. and Phys.* VIII. 207). The experiments were made upon the same dog and after the same manner as their previous ones, with the aid of their respiration apparatus. The gain of fat in the body was estimated by the authors from the deficit of carbon in the excreta as opposed to the quantity taken in, and this in reference to the excretion of N, which represented the measure for decomposition of the albuminous bodies. The results are the following: A great quantity of starch can be changed into grape-sugar and absorbed from the intestinal canal of the dog. As a minimum one kilo dog digests 15 grms. starch in 24 hours, whilst one kilo fattened ox, with copious feeding, did not absorb more than 12,7 grms. The assumption therefore that the dog, as a flesh-eater, is not suited for experiments with feeding on carbohydrates is not correct. Even when great quantities of starch were given, the quantity of fæces excreted was inconsiderable, and consisted, for the most part, of the residue of the intestinal juice, and not till the quantity of starch exceeded 379 grms. in the 24 hours did unchanged starch appear in the fæces. The sugar absorbed split up completely into CO_2 and water. The quantity of fat formed is not proportional to the quantity of carbohydrates given. On the contrary, the quantity of fat stored up depends upon the quantity of flesh present; then with 379 grms. starch and 1800 grms. flesh it was 112 grms., with 800 grms. flesh and 379 grms. starch only 55 grm. From the N. excreted during exclusive feeding with starch, it can be calculated how much flesh the animal has given up from its body. Leave this aside, and calculate how much fat must be stored up when flesh and starch were given, and a tolerably exact agreement with the number obtained by the experiment is got. This shows that the fat arises from the albumen of the flesh. Of course a certain proportion must always exist between the quantity of carbohydrates introduced, and of the fat formed, in that the starch protects from consumption the fat resulting from the decomposition of the albumen. This is the only action of the

carbohydrates; a formation of fat from them was not found throughout. From their former experiments the authors calculated the separation of 11, 22 grms. fat from 100 grms. flesh. If large doses of starch are added to the flesh, so in fact almost 11 per cent. of flesh as fat will be added.

Recherches Expérimentales sur la Question de Savoir, si certaines Cellules des Glandes (dites à pepsine) de l'Estomac présentent une réaction Acide.—R. Lepine (*Gazet. Med.* No. 51, 1873) placed thin sections of the mucous membrane of the stomach in a mixture of ferrocyanide of potassium and sulphate of iron, which was treated with so much caustic potash, that the precipitated Prussian blue was again dissolved with a yellow colour. Such a fluid produces, with a trace of free acid, the separation of Prussian blue. In these experiments with microscopic investigation nowhere in the glands of the stomach was the separation of Prussian blue detected. The experiment was varied in another way. Pieces of the mucous membrane several Cm. long were carefully removed and used as a small dialyser. On the one side was placed lactate or sulphate of iron in watery alcoholic solution, and on the other side ferrocyanide of potassium. The free surface of the mucous membrane was directed at one time upwards, at another downwards. The result was in all cases the same. The formation of Prussian blue occurred, but exclusively on the free surface. In the glands of the stomach themselves no separation of the Prussian blue was to be detected, and the author concludes from his experiments that the acid of the gastric juice is not formed in the cells of the glands.

Movements of the Œsophagus.—Mosso (Movimenti dell' Esofago, Torino, 1873, pp. 44) employed the following simple methods for studying the movements of this tube. The œsophagus was exposed in the neck and into it a small wooden ball of the size and shape of an olive was introduced. A thin flexible wire was attached to the ball, so that it could be easily extracted from the stomach. The experiments were performed on cats and dogs. A transverse section of the œsophagus does not hinder the propagation of the peristaltic movements when the upper half of the tube is stimulated. Excision of a part of the œsophagus, e.g. from under the larynx to the arch of the aorta, does not arrest the peristaltic movements from above downwards, under similar conditions. Even when the œsophagus was ligatured two or more times over a cylinder of wood introduced into it (the ligatures were separated about 1 cm. from each other), the movements were propagated from above downwards.

In a second series of experiments, on stimulating the vagi by the opening shocks of an ordinary induction apparatus, the contractions of the œsophagus formed a gradually ascending 'Treppe,' such as Bowditch has described for the heart. The excitability of the vagi continued exceedingly long after death. A dog showed, four and a half hours after death, these movements when the recurrent nerves were stimulated. The movements of the lower part of the œsophagus,

described by Magendie and Schiff, have not been observed by the author. He finds that the lower part of the œsophagus is always at rest, except when the upper portion is stimulated and caused to contract.

MOVEMENTS OF THE INTESTINES.—V. Basch (*Die Hemmung der Darmbewegung durch den N. Splanchnicus, Sitzungsb. der k. Akad. der Wissensch.* LXVIII. June, 1873). Basch and Oser have previously shown that nicotine, at a certain stage, produces regular peristaltic movements of the intestines. The author employed the movements so produced for studying the inhibitory action of the splanchnicus on the movements of the intestine. The experiments were performed upon dogs. Not only the phenomena exhibited by the intestines were observed, but the blood-pressure was measured at the same time in the carotid (this *Journal*, VIII. 226).

Stimulation of the splanchnicus only inhibits the intestinal movements, when it, at the same time, produces a considerable increase in the arterial blood-pressure. The time when the intestines come to a standstill always corresponds to the time when the blood-pressure has reached its highest value, and this equally whether the stimulation acts for a longer or shorter time. The intestinal movements could be inhibited when the medulla oblongata was stimulated electrically, after previous section of both splanchnici. Also in these experiments the inhibitory action occurred only when at the same time the arterial blood-pressure was considerably increased—i.e. in animals with divided splanchnics, where not all the nervous connections between the vaso-motor centres and the vessels of the abdomen were divided. Asphyxiated blood or nicotin, after previous section of the splanchnicus, also inhibited the intestinal movements. It is therefore certain that the splanchnicus is not the only inhibitory nerve for the intestinal movements, but that also this inhibition is always accompanied by the contraction of the vessels of the intestines, and that the inhibitory action is to be considered as a function of the vaso-motor properties of the splanchnicus. Van Braam Honckgeest (*Pflüg. Arch.* VIII. 163), in opposition to V. Basch, concludes from his experiments, that the inhibitory function of the splanchnicus is specific and distinct from its vaso-motor functions, and as supporting this view he finds that, 1. After section of the splanchnicus the spontaneous movements become more lively than before. 2. Stimulation of the vagus after section of the splanchnicus produces constant movements. 3. On exposing the intestines freely to the air, hyperæmia, in consequence of paralysis of these vessels, i.e. paralysis of the peripheric expansions of the splanchnicus, is produced; then the spontaneous movements are neither so lively, nor is stimulation of the vagus followed by movement, or, if so, only by very weak and irregular movements in some loops of the small intestines. 4. During the inhibition from stimulation of the splanchnicus, the intestines do not become pale, but remain hyperæmic as before. Blood-pressure not measured.

Liver.

"Lectures on Diabetes and the Glycogenic Function of the Liver." M. Claude Bernard. *Lond. Med. Rec.* Nos. 40, 41, 42, 43, 44, 45, 46, 47, translated from the *Revue des Cours Scientifique.*—"Lussana and others on the Portal Circulation and the Biliary Secretion." Abstracts by Dr Brunton, in *Lond. Med. Rec.* II. No. 56.—"The formation of Glycogen in the Liver." B. Luchsinger, *Pflüg. Arch.* VIII. 289. (Abstr. in *Centralblatt*, No. 10, 1874.)

BILIARY FISTULA. H. Westphalen, *Deutsch. Arch. f. klin. Med.* 1873, XI. 588 (Abstr. in *Centralblatt*, No. 49, 1873). The bile was collected from a case of empyema of the right side, in which paracentesis had been performed. At first the pus was mixed with bile and had a bad odour, but fourteen days after the operation, whilst the fæces were devoid of colour (this lasted three weeks), only pure bile was obtained. The bile was collected by a special apparatus. The bile was quite fresh, of a bright gold yellow colour, neutral or scarcely alkaline, and of a thinly fluid consistence. The smallest and most concentrated quantity was evacuated between 2—4 A.M. In 24 hours a mean of 498,85 grms. was evacuated. Weight of patient 67,5—64 kilos. S. G. 1008—1012,5. Percentage in solid residue 2,24—2,28 per cent.

The dried bile yielded with ether, cholesterin 2,49 per cent., unsaponifiable fats with some oleate of soda 0,44 per cent. Lecithin (reckoned from the P. per cent) 0,21 per cent. The substances insoluble in ether and alcohol (coloured mucin) made up 10 °/₀ of the solid residue.

The following table gives the results of the chemical analysis of the ash of the bile.

	In 100 parts Ash.	In 100 parts dried Bile.	In 100 parts fluid Bile.
K Cl	3,39	1,276	0,029
Na Cl	65,16	24,508	0,557
$CO_3\ Na_2$	11,11	4,108	0,095
$PO_4\ Na_3$	15,90	5,984	0,136
$2\,(PO_4)\,Ca_3$	4,44	1,672	0,038
	100,00	37,620	0,855

The alcoholic extract of the dried bile contained, glycocholate of soda 4,48 per cent., palmitinate and stearinate of soda 6,4 per cent. In the decomposition of these with baryta, only glycin and no trace of taurin was obtained.

Urea, sugar, or other substances capable of reducing an alkaline solution of Cu, were not found in the bile; only several times traces of albumen and leucin (no tyrosin), of bile-pigments, bilirubin and biliverdin, and in the ash in three tests each time some Cu, quinine or calomel taken did not pass into the bile. The latter had no influence

whatever on the quantity of bile. The same was the case with water, which however doubled the quantity of urine and diminished its S.G. from 10,21 to 10,11.

On the Albuminous Substances of the Hepatic Cells. P. Plósz (*Pflüg. Arch.* vii. 371), under Kühne's direction, has investigated this subject. It is well known that the liver-cells undergo marked changes soon after the removal of the organ from the body. Sections of the fresh liver, even when free from blood, always have an alkaline reaction; but if kept for a short time at ordinary temperature, and still more quickly when at blood-temperature, they become acid. The tissue of the liver as a whole becomes also more resistant and rigid (Kühne). The change in character of those cells, which have become altered by post-mortem changes, or, as the author terms them, cells in a state of rigor mortis, are described. To examine these, all blood, bile, and lymph must be removed from the liver by injections of weak solution of NaCl (0,75 $^0/_0$). The liver after being washed for an hour or two appears pale, and neither hæmoglobin nor glycogen are to be detected in it. Some, not inconsiderable quantity of soluble albuminous compound is also diffused out and washed away. The liver is then finely minced, and passed through fine linen. The pap is treated with a 0·75 $^0/_0$ solution of NaCl, and set aside for the cells to precipitate. The supernatant fluid can be drawn off in a few hours, and is found to be opalescent, partly owing to the presence of a little glycogen, and partly owing to extremely fine granules which proceed from broken up cells. When these have settled, or after filtration, the clear fluid is found to contain—1. An albuminous compound coagulating at 45° C., and not redissolving at a higher temperature, soluble in solutions of NaCl, sulphate and carbonate of soda, HCl, and acetic acid. It closely resembles the albuminous substance found by Kühne in muscular fibre. 2. An albuminous nuclein combination, which coagulates at 70° C., and only differs from the preceding in this point. It agrees in all its characters with the albuminous compound found by Miescher in the nuclei of pus-cells. If the cells exhausted with the 0·75$^0/_0$ solution of NaCl, be now treated with a 10$^0/_0$ solution, a considerable quantity of an albuminous substance is obtained, coagulating at 75° C., which is precipitated by the addition of much water, and by an excess of a concentrated solution of NaCl, and presents other features showing that it belongs to the globulin-like albuminous bodies, and closely agrees with myosin. The liver-cells from which the above albuminous substances have been extracted yield nuclein (*Lancet*, Oct. 18, 1873).

Formation of Glycogen in the Liver. G. Salomon (*Centralblatt*, No. 12, 1874) employed rabbits for his experiments. They were fed on different substances, and the glycogen in the liver estimated by Brücke's process. He confirms Luchsinger's statement as to the difficulty of extracting glycogen and sugar completely from the liver. The author confirms Hoppe-Seyler's view, that on feeding with gelatine, glycogen is formed in the liver. This "gelatine-glyco-

gen" had the same reaction as ordinary glycogen. Under feeding with neutral fat (olive oil) and dilute glycerine, in each case glycogen was found in the liver. Milk-sugar and fruit-sugar yielded similar results.

On the Hæmatogenous Formation of the Colouring Matter of the Bile.—Steiner (*Reichert und Du Bois' Archiv*, 1873, p. 160), after referring to what is already known upon this subject, cites the results of his own experiments upon the injection of water (at the temperature of the body) into the carotid artery and external jugular of rabbits. He finds that an injection of 20 cc. of distilled water into the common carotid is not followed, either by a solution of the red blood-corpuscles in the urine, or by the presence of the colouring matter of the bile in the same. From 17 other experiments the author finds, after the injection into the right or left external jugular, of from 30 to 50 cc. of distilled water, at the temperature of the body, that in twelve cases there was a solution of the blood-corpuscles in the blood-vessels, *i. e.* the appearance of free hæmoglobin in the urine. After precipitation of the albumen and examination of the filtrate, in no case was the colouring matter of the bile proved in the latter. The method of precipitating the albumen by heat, and examining the filtrate for the presence of bile-pigment, is quite reliable. These results are completely negative, and stand in direct opposition to the positive results of M. Hermann, who, however, operated upon dogs, and collected the urine by canulæ introduced into the ureters.

Production of Icterus.—Audigné (*Gaz. Méd. de Paris*, No. 1, 1874) read a communication on mechanical icterus. He ligatured the bile-duct in a dog. The animal lived 19 days, and though it continued to have a voracious appetite, it emaciated visibly. The colouring matter of the bile was found four hours after the operation in large quantity in the urine, and on the second day the fæces were quite discoloured. The icteric tint in the skin and conjunctiva appeared on the eighth day. The appearance of the bile-pigment in the urine is at variance with the results of Frerichs, who observed this only after twenty-four hours. A histological examination was made of the liver.

Changes in the Liver which follow Ligature of the Bile-Ducts.—J. W. Legg (*St Barthol. Hosp. Rep.* IX. 1873) operated upon cats. The animals survived the operation for varying times up to 20 days, and peritonitis when present remained local. This author also, in opposition to Frerichs, observed the icteric tint of the conjunctiva only on the 10th, nay even on the 14th day, after the operation. Hein. Mayer also obtained a similar result upon operating on cats. The cats became emaciated and died without convulsive phenomena, and only became comatose shortly before death. With regard to the cause of death, the author lays stress upon the decided diminution or absence of glycogen of the liver (tested with iodine solution). Sugar was not found in three cases where a watery extract of the liver was made; still in one case, on the sixth day after

the ligature of the bile-ducts, diabetes was produced by puncturing the floor of the fourth ventricle. If the animals lived after the fourth day, a decided increase in the connective tissue of the liver was found on histological examination. The cells of the liver were atrophied or infiltrated with fat, but not more strongly pigmented than normal. In the kidney, as a rule, the epithelial cells were cloudy or had undergone fatty degeneration.

GENITO-URINARY SYSTEM.—"Ascertained Facts regarding Diabetes and Hydruria." Résumé by Dr Brunton in *Lond. Med. Rec.* I. No. 42.——"Erection in Birds." C. Eckhard, *Centralblatt*, No. 53, 1873. ——"The Carbonic Acid in the Urine in Fever." C. A. Ewald, *Reich. und du Bois Reym. Arch.* 1873.——"Elimination of Urea." Rabuteau, *L'Union Médicale*, 1873, No. 107.——"Estimation of the quantity of Albumen." Esbach in *Société de Biologie* (*Gaz. Méd. de Paris*, No. 5, 1874).——"Condition of the Circulation in the Kidney." A. Högyes, *Arch. f. Exp. Pathol.* 1873, I. 289. Abstract in *Centralblatt*, No. 1, 1874).——"Influence of the activity of the Skin on the Secretion of Urine." K. Müller, *Arch. f. exp. Pathol.* 1873, I. 429. Abstract in *Centralblatt*, No. 3, 1874.

PHYSIOLOGY OF THE COLLECTION OF URINE IN THE BLADDER.—G. Edlefsen (*Pflüg. Archiv*, VII. 499) took a sufficient quantity of water before going to bed, and evacuated the contents of his bladder at stated periods (7 times), and then took the sp. gr. of each portion. The first portion evacuated in the morning showed the highest sp. gr. The sp. gr. of those portions following became gradually less. The layers of urine in the bladder became mixed during the day, owing to the movements of the body etc. If the author evacuated his bladder while on his hands and knees, the first portion in the morning had the lowest sp. gr., and the last portion the highest. The gentle motion of the body had not changed the position of the contents of the bladder. The author's experiments show that the walls of the bladder move round its contents during the movements of the body.

TO PRODUCE GLYCOSURIA.—Ewald (*Centralblatt*, No. 52, 1873) injected subcutaneously from 0,5 to 2,0 grms. of nitro-benzol (sp. gr. 1,078) into a rabbit. On pressing on the bladder to collect the urine during the first three hours after the operation, a substance having all the properties of sugar was found in the urine. This substance can also be obtained plentifully till 20 hours after the injection, after that it gradually disappears, and after 24—36 hours it can no longer be detected. The author could not produce mellituria in dogs on injecting nitro-benzol subcutaneously; but when the drug was given by the mouth large quantities of sugar (in one case 2·5 per cent.) were found.

COMPARATIVE OBSERVATIONS ON THE CONSUMPTION OF SUGAR IN DIABETIC AND NON-DIABETIC ANIMALS.—L. Seelig, *Inaug. Dissert.* Königsberg, 1873. (From Abs. in *Centralblatt*, No. 59, 1873.) The author experimented on diabetic and non-diabetic rabbits, both of

which had been allowed to hunger. Diabetes was produced by Eckhard's method. The author then convinced himself that in the starving animals after the diabetic sugar had disappeared from the urine or occurred only in traces, corresponding to the results of Dock (the hunger period lasted 3—5 days, the collected urine was evacuated by pressure, after it had collected for six hours), that only once by tibiation with Fehling's solution did he find 0·4 grms. of sugar. A solution of sugar (generally 20 ccm. of a 10 per cent. sol. = 2 grms. sugar) was then injected into the jugular vein, in the one case into the starved animals, and in the other into the starved diabetic ones. In the former case only traces of sugar appeared in the urine, when the animals had starved for 5, 6 and 7 days, somewhat more, when the hunger-period was shorter. As a mean of 16 experiments, 0·2 grms. of sugar were found in the urine, while in the diabetic animals the quantity of sugar excreted was much greater; the maximum 0·925 grms., the minimum 0·27 grms. (in this case only 0·91 grms. were injected), mean 0·6 grms. The mean quantity of urine in non-diabetic animals was 25 ccm., in the diabetic 41 ccm. The diabetic animal, therefore, is distinguished from the non-diabetic one by its incapability of using the sugar for the nutrition of its body. For the theoretical deductions we must refer to the original.

Reaction of Urine and Sweat.—A. Moriggia (*Moleschott's Untersuch.* 1873, IX. 129), from experiments upon himself and animals, found that the sweat of plant-eating animals is generally alkaline, that of flesh-eaters acid. In animals and man the urine becomes acid during fasting or under flesh diet, under vegetable food alkaline; but the sweat has a reaction peculiar to the individual. To change the reaction of the urine, it is only necessary to have a long and continued change of the diet.

Anatomy and Physiology of the Kidney.—R. Heidenhain, *Arch. f. Mikroscop. Anat.* x. 1.—This paper is chiefly devoted to the histology of the secretory apparatus of the kidney. The author describes peculiar rod-shaped bodies (*Stäbchen*) in the contorted tubules and in the broad part of the loops of Henle. Experimenting with indigo-sulphate of soda (that bought in the shops is generally impure), the author concludes that the kidneys are the specific secreting organs for this substance, in the same sense as they are for urea. In this process the Malpighian capsules do not act a part, the excretion is accomplished by the tubuli contorti. The single tubuli contorti can act independent of each other, so that in one a living secretion may take place, whilst in those in its immediate neighbourhood no secretion is to be observed. The straight urinary tubules do not excrete the indigo-sulphate, but serve only for conducting the formed secretion. Not all the constituents which occur in the urine are secreted in the Malpighian capsules, the author agreeing with Bowman that the capsules secrete only water, and perhaps salts of small atomic weight; whilst he thinks that the secretion of urea devolves upon the tubuli contorti. What is true of the contorted tubules, is also true of

the broad part of the loops of Henle. Rod-shaped bodies were also found in the fine ducts of the nasal, parotid, and sub-maxillary glands, but not in those of the sub-lingual. In animals which excreted blue urine on stimulating these glands electrically, no blue secretion was obtained. [See fuller abstract in *Lond. Med. Recd.* I. 823.]

Uterus.

ON THE INNERVATION OF THE UTERUS.—Scherschewsky, under Cyon's direction, has arrived at the following results (*Pflüger's Archiv*, VIII. 349). 1. The uterine plexus contains the most important, if not the only motor nerves, which can produce actual movement of the uterus, on stimulation of their peripheral ends (stimulation of the central ends produces only violent vomiting). 2. Stimulation of the central ends of the first two sacral nerves produces reflexly violent uterine movements, which disappear after previous section of the uterine plexus (stimulation of the peripheral ends produce only violent contraction of the urinary bladder and rectum). Stimulation of the brachial, crural, median, sciatic, &c., nerves, does not produce peristaltic movements of the uterus, but only a slight rigidity and paleness of the organ. 4. The consequences of stimulation of these nerves disappear when the aorta is previously tied. Stimulation of the central ends of the sacral nerves is still accompanied by peristaltic movements of the uterus, after ligature of the aorta.

Muscle.

"Shortening of Muscle and Tendons." Hermann, *Pflüg. Arch.* VII. 417 (*Journ. of Anat. and Phys.* VIII. 214), M. W. Englemann, in *Pflüg. Arch.* VIII. 77, and Reply by Hermann at 275.——"Difference of the Physiological Action of Induced Currents, according to the Nature of the Wire forming the Secondary Spiral." Onimus, *Acad. d. Scien.* in *Gaz. Méd. de Paris*, No. 1, 1874.——"Das Myophysische Gesetz." W. Preyer, Jena, 1874, pp. 144; critical and experimental observations on the same. B. Luchsinger, *Pflüg. Arch.* VIII. 538.——"On two Electro-Physiological Disputed Points," A. Grünhagen, *Pflüg. Arch.* VIII. 519.

RED AND WHITE MUSCLES OF THE RABBIT AND RAY.—Ranvier, *Comptes Rendus*, LXXVII. 1105. It is well known that certain animals have two kinds of voluntary muscles, the red and the pale. Thus the semi-tendinosus in the rabbit is red, whilst the vastus internus in which it is lodged is pale. In the rays and torpedoes there are muscles formed of the two kinds of fibres. The difference of colour does not depend upon the quality of blood in the capillaries. On stimulating the semi-tendinosus of a rabbit with an interrupted current, it contracts gradually and progressively, when tetanised it remains contracted as long as the irritation is continued, and returns gradually to its original length on cessation of the irritation. A pale

muscle (adductor magnus) of the rabbit stimulated by the same current contracts quickly and abruptly, and on cessation of the stimulation returns as abruptly to its original length. The red muscles of the rays and the few pale fibres found under the skin of the back, exhibit similar physiological properties. These two kinds of muscles exhibit slight differences in histological structure. A more complete description of the physiological properties and histological structure of these two kinds of voluntary muscle is given by Ranvier in the *Arch. de Phys.* VI. 1. The time which elapses between the beginning of the stimulation and the contraction of the pale muscle is $\frac{1}{83}$ of a second, whilst for the red muscles with the same rapidity and intensity of the stimulating current the time occupied is $\frac{1}{18}$ of a second, or about four times as long a period of latent contraction or irritation as for the pale muscle.

As a corollary to the above the important histological fact may be added, that Ranvier (*Soc. de Biologie*, *Gaz. Méd. de Paris*, No. 4, 1874) has found that the transverse branches connecting the parallel capillaries in the red muscles are provided with small dilatation-ampullæ, or little aneurysmal-like sacs. The importance of this arrangement upon the circulation in muscle is pointed out by the author.

DEVELOPMENT OF MUSCULAR FIBRES IN THE FROG.—Petrowsky (*Centralblatt*, No. 49, 1873) has arrived at the following results:

1. In the larval stage the muscle consists of fusiform fibres with a series of oval nuclei in the middle. Neither sarcolemma nor peripheral nuclei are present.

2. On the periphery of several fibres, at the end of the larval stage, and in most of the fibres of the frog when 10 mm. long, the nuclei and sarcolemma appear at the same time together. These nuclei appear for the most part as rods, but are oval nuclei, when seen in profile.

3. The axial series of nuclei disappear, the greatest part of the peripheral nuclei separate from the sarcolemma, increase by division, and thus form rows of parallel peripheral nuclei. Increase of the fibres goes hand in hand with increase of the rows.

4. In the increase of the muscles the formation of new fibres participates, but the formation of new fibres does not take place through division of the older fibres.

RELATIVE PROPORTION OF NERVE TO MUSCULAR FIBRES.—P. Tergast (*Arch. f. Mikroskop. Anat.* Bd. IX. 36) gives the following as the result of his investigations upon the relative proportion of nerve-fibres to muscular fibres in the ocular muscles of the sheep. The proportionate number of primitive nerve-fibres to muscular fibres varied considerably in the several muscles. In the obliquus inferior there are three or four muscular fibres for each nerve-fibre, in the obliquus superior six or seven, in the rectus inferior seven or eight, and in the externus ten. In the ocular muscles of man there appear to be three primitive nerve-fibres to every seven muscular fibres. In the

general muscles of the body the proportion of nerve-fibres is much less. In the biceps of a young dog the author found only one primitive nerve-fibre to eighty-three muscular fibres, whilst in the sartorius they were in the ratio of one to forty or sixty muscular fibres.

Action of Nitrite of Amyl on Muscle.—R. Pick (*Centralblatt*, No. 55, 1873) shows that this substance produces a rapid and direct paralysis of muscular fibre.

Bone.

"On the Marrow of Bone." Ch. Robin, in *Journ. de l'Anat.* Jan. and Feb. 1874, 33.——"Bone Absorption by means of Giant-cells." A. Morison, *Edin. Med. Journ.* ccxx. 305.——"Intercellular Growth of Bone." Schachowa, *Centralblatt*, No. 57, 1873.——"Resorption of Bone and Giant-cells." Rustizky, *Virch. Arch.* lix. 202.——"Composition of Bone under Diet poor in Lime or Phosphoric Acid." H. Weiske and E. Wildt, *Zeitsch. f. Biol.* ix. 541. (Abst. in *Centralblatt*, No. 14, 1874.)

Feeding with Madder.—*Centralblatt*, No. 47, 1873.—Strelzoff in his experiments with this substance has arrived at the following results: 1. The bones of old as well as of young pigeons are coloured by madder. The coloration occurs more quickly in young than in old pigeons. 2. The bones of very old pigeons are either not at all or but slightly coloured on feeding with madder. 3. Not only the osseous tissue formed during the feeding with madder, but all that formed previously is coloured by the madder. 4. A juice-canal system which is in connection with the processes of the bone-corpuscles, is intercalated between the blood-vessels and the osseous tissue, and may be regarded as a lymphatic system. 5. The bones are coloured in the direction of their juice-canals by the madder.

Artificial Arrest of the Growth of Long Bones by Irritation of the Epiphyses.—A. Bidder (*Archiv f. exper. Pathol. und Pharmacol.* 1873, i. 248. Plates vii. and viii.) operated on the superior epiphysial cartilage of young rabbits. The cartilage was either exposed and transfixed with needles or destroyed by section. Growth of the bone was arrested either on one side only or over the whole extent of the terminal surface, according to the part irritated, and this effect was marked throughout the whole length of the bone as far as the distal epiphysis. Destruction of the cartilage on the fibular side was followed by growth of the opposite side, causing curvature of the bone with the convexity inwards.

Contributions to the Chemistry of Bone.—R. Maly and J. Donath (*Sitz. d. Wien. Acad. d. Wissensch.* lxviii. ii. Abth. Juni-Heft, 1873) made experiments upon the solubility of bones and phosphate of lime in different fluids. They examined three preparations: 1. Phosphate of lime precipitated from lime-water, by the addition of phosphoric acid and preserved under water. 2. Phosphate of lime

from chloride of calcium, ammonia, and phosphate of soda, dried and ignited. 3. Powdered bones purified with alcohol and ether. 100,000 parts of water dissolved with shaking and long standing, of the first preparation 2·36, of the second 2·56, and of the third 3·00. Under certain circumstances salts in the water increased its power of solubility: 100,000 parts of a 1 per cent. solution of chloride of ammonium dissolved 16·8 of bone-powder. Pieces of the femur of an ox, after lying several days in 2 per cent. solutions of different substances, lost most in weight in water rich in Co_2, then in sal-ammoniac, bile, common salt, simple water. Some substances diminish the power of water for dissolving phosphate of lime, sugars, gelatine, glycerine, lactate of soda, &c. Co_2 dissolves phosphate of lime easily. Liberal potations of water increase the quantity of phosphoric acid excreted per urethram—a fact already well known. No increase, however, of this acid was noted under the use of water impregnated with Co_2. The authors then discuss the question whether the calcareous salts in bone are really in chemical combination with the ground substance of the bone or not. They cite the facts that are favourable to such a view, and then give experiments of their own. The authors tried to impregnate bone-cartilage with phosphate of lime (after extracting the bones with HCl), but in vain. They added to solutions of gelatine of different concentrations ammoniacal solution of phosphate of soda and chloride of calcium solution, and each time so much, that therefrom 1·96 grms. dry phosphate of lime $Ca_3\,(Po_4)_2$ must arise. The precipitates always contained gelatine, but the quantity varied with the concentration of the gelatine solution; from 1·96 grms. of phosphate of lime 0·37, 0·47, to 0·67 grms. of gelatine were obtained, proportions which distinctly speak against a chemical combination of the gelatine with the phosphate of lime. Other gelatine-containing precipitates (oxide of iron, silicic acic, zinc oxide) produced in gelatinous solutions, also contained gelatine, often in a high degree; thus the iron oxide contained 51·8 of gelatine, and the zinc oxide 47·8 per cent.; other substances had also this property, as albumen of egg, whose precipitate contained 32·4 of organic substance, that of gum 27·7 per cent. The phenomenon is therefore a purely mechanical one, but the mixture is a very intimate one, so that even treatment for days with hot water does not serve to extract all the gelatine. In complete unison with the above is the fact that phosphate of lime in bones conducts itself with regard to solvents in exactly the same way as precipitated phosphate of lime. As supporting the mechanical nature of the union of phosphate of lime in bone, see "Relation of Bone-Cartilage to Phosphate of Lime." By C. Aeby, in *Centralblatt*, No. 44, 1873.

PRODUCTION OF RICKETS ARTIFICIALLY.—L. Tripier (*Arch. de Phys.* VI. 108) has tried to produce rickets artificially in young animals. Guérin has answered this question in the affirmative. He fed young dogs on raw flesh, instead of the milk of their mother. Tripier has made similar experiments on young cats and dogs, and fowls, giving them insufficient nutriment, keeping them both in the town and country to

test the effect of hygienic conditions. In no case was any affection of the skeleton observed. Careful analyses were made of the bones of the animals experimented on.

Milk.

PERCENTAGE OF FAT IN HUMAN MILK.—Brunner (Abs. in *Journ. of Anat. and Phys.* VIII. 210) gave as the mean percentage 1·73. A. Schukowsky (*Zeitsch. f. Biologie,* IX. 432), from numerous analyses which he has made in Moscow, is convinced that this figure is too low. He finds that the normal milk of a healthy woman, suckling her child, contains very seldom less than 3 per cent. of fat. Brunner estimated the fat by Trommer's method.

EXCISION OF THE MAMMÆ DURING LACTATION.—Sinéty read a communication before the *Société de Biologie* (*Gaz. Méd. de Paris,* No. 3, 1874) upon this subject. Rabbits and dogs did not survive extirpation of all the glands. Guinea-pigs were therefore employed. The guinea-pigs operated on in September are still alive. The removal of these glands is followed by the occurrence of sugar in the urine; under the microscope globules of fat are to be detected. In a further communication (*Gaz. Méd.* No. 8, 1874) the author finds that the absence of the mammæ has no effect on the triple function of fecundation, gestation, and parturition. Further, that in female guinea-pigs, deprived of these organs, glycosuria is not produced during gestation or after parturition. Gestation is without influence on the appearance of sugar in the urine, this phenomenon depending entirely on lactation.——"Reaction of Milk to Litmus." Vogel, *Jour. f. Pract. Chem.* N. F. VIII. 137.

Temperature.

"Increase of Temperature by Pyrogenic Substances." S. Dobczcanski and B. Nannyn, *Arch. f. Exp. Pathol. u. Pharmac.* 1873, I. 181.——"Effects of Alcohol on Warm-blooded Animals." Read before the British Association at Bradford. Abstract in *Nature,* IX. 132.——"Temperature in Health." Ed. Laurie. *Indian Med. Gazette.* Abstract in *Lond. Med. Rec.* No. 46.——"Influence of Alcohol on the Bodily Temperature." F. Riegel, *Deutsch. Archiv f. Klin. Med.* 1873, XII. 79.——"Temperature of the Heart and Lungs." Albert and Stricker, *Wien. Med. Jahrbuch,* 1873, 29. Abst. in *Centralblatt,* No. 3, 1874.——"Influence of Nerve Lesions upon Temperature." W. Mitchell, *Arch. of Scient. and Pract. Med.* 1873, 351.——"Calorification in Asphyxia." Claude Bernard, *Lancet,* Oct. 3, 1873.——"Doctrine of the Regulation of the Temperature." F. Riegel, *Virch. Arch.* LIX. 114.——"Anæsthesia from Cold." Horvath, *Centralblatt,* No. 14, 1873.——"Action of Alcohol on the Temperature and Pulse." Rabow, Abs. in *Centralblatt,* No. 21, and Daub in *Centralblatt,* No. 29, 1873.——"Effects of Exercise on the Temperature and Circulation." C. H. Jones, *Proceeds. of Roy. Soc.* XXI. 374.——"Effects of Exercise on the Bodily Temperature." Allbutt, *Jour. of Anat. and*

Phys. VII. 106.——"On the Substances which Increase the Temp. of the Animal Body." P. Lewitzky, *Centralblatt*, No. 46, 1873.

INSENSIBLE EXCRETION IN FEVER.—Fr. Neumann (Exp. Untersuch. über das Verhalten der insensibelen Ausgabe im Fieber), *Inaug. Dissert.* Dorpat, 1873. From experiments upon dogs the author arrives at the following results: 1. During hunger the insensible excretion sinks from day to day. Variations caused by the varying conditions of the atmosphere occur. 2. During the fever the insensible excretion was in general greater than without fever under the same conditions. 3. A parallelism seems to be present between the degree of temperature and the insensible excretion. 4. During and after the decrease of the fever ("crisis and epicritical stage") an increase of the insensible excretion takes place.

ON THE FORMATION AND REGULATION OF ANIMAL HEAT (Del Potere regulatore della Temperatura Animale, Studio Critico-sperimentale, by A. Murri, pp. 79. Firenze, 1873). This is an exceedingly able paper. We have only space to cite a few of the author's more important results. A fuller abstract will be found in the *Lond. Med. Rec.* No. 47. 1. The increase in the excretion of Co_2 during a cold bath is probably the consequence of more complete expiration, and even if increased formation of the same was demonstrated, this can be explained without increased production of heat, which is not the case. 2. The increase of temperature in the axilla during the cold bath is quite compatible with a considerable diminution of the quantity of the heat in the body. There is actually a diminution of heat. It is, therefore, not necessary to assume an increased production. 3. The calorimetric measurements of Liebermeister and Kernig are not exact, because the cooling which the body experiences in the cold bath was deduced from erroneous calculations. The direct proof shows that no more heat is produced in the cold bath than under ordinary circumstances. 4. The hypothesis that there exists a nervous centre which fixes the temperature in health and disease is quite untenable. The heat-regulating centres are neither proved nor rendered probable. 5. Fever is produced by a series of unusual chemical changes (fever-process) which thus increase the heat-production, so that the bodily temperature rises. Even continued increase of temperature, which is not based on a special unusual biochemical process, is not feverish (e. g. hysterical, epileptic, and tetanic convulsions).

ACTION OF SWEAT.—A. Röhrig, *Jahr. f. Balneol.* 1873, I. 1, collected sweat from his forehead, and injected 3·5 cc. of it, fresh and filtered, into the jugular vein of a rabbit at mid-day. The temperature of the animal was 37·2° C. before the injection. Towards evening the temperature rose, and during the night reached 40·2° C. The heart-beats rose from 122 per min. to 326, and the respiration from 82 to 105. Next morning the temperature was 40·2° C. heart-beats 315, and respiration 215 in the minute. In two days the animal

returned to the normal. Urine during the fever contained albumen, which disappeared as the fever subsided. What elements in the sweat produced these effects the author has left undetermined.

INFLUENCE OF VARIOUS DEGREES OF TEMPERATURE ON THE IRIS OF MAMMALS AND UPON THE STRIATED MUSCULAR FIBRE OF THE FROG.—Grünhagen (*Tageblatt der* 46. *Versam. deutsch. Naturf. und Aerzte in Wiesbaden*) stated that certain temperatures between 0° C. and blood-heat exert a considerable influence on the size of the pupils in mammalia after extirpation. In the cat, if the eye after death is exposed to the temperature of the blood, the pupils remain widely dilated; whilst, if temp. is lowered to the mean temp. of the room, they contract strongly, and dilate again strongly when the temp. is reduced to 0° C. The author believes that this is not due to contraction and dilatation of the sphincter pupillæ, as Brown-Séquard and H. Müller are inclined to believe, but that the cause of the phenomenon in question is due to the varying capacity of the tissue of the iris for the imbibition of water at different temperatures. He is of opinion that the tonus of the tissue of the iris is lost after death by the absorption of water; whilst on exposure to low temperature (0° C.) it is restored by the giving up of water. That water is really given off in the latter case is rendered probable by the reaction of the lens to cold, for this body becomes cataractous at a freezing temp., owing to the formation of vacuoles in its substance. The impaired irritability of the iris can be restored even after two days by placing the iris in a chamber at blood-heat. The author also showed that on cooling down the muscular tissue of the frog to the freezing point, its irritability, upon the application of a mechanical stimulus, was greatly increased. In this we possess a means of producing muscular contraction mechanically, without the participation of nerves. The contractions so produced can produce secondary contractions.——"On the Substances which increase the Temperature of the Animal Body." P. Lewitzky in *Centralblatt*, No. 46, 1873.——"Increase of Temperature in Fever." Dobczcanski and B. Nannyn. *Arch. f. exp. Path. and Pharmac.* 1873, I. 1. 181.

ON THE HEAT PRODUCED IN THE BODY, AND THE EFFECTS OF EXPOSURE TO COLD.—Draper (*American Jour. of Scien. and Arts*, Dec. 1872) wished to determine the quantity of heat passing from the surface of the body by finding how much it would elevate the temperature of a known mass of cool water during a given period of time. By lying quietly for one hour in a bath of 74° F., enough heat was lost from the body to raise the temperature of the water 2° F. and to lower that of the body 1° F. (temp. measured in mouth and axilla). The volume of water in the bath was 7½ cub. ft., and that of the body 3, and the author concludes that enough heat is evolved from the body in one hour to warm the body itself 5° F. Thermometers in the mouth and axilla indicate a steady fall of temperature during the bath and for a short time after, leaving it accompanied by a diminished rate of respiration and pulse. When the experimenter executed con-

tinued muscular movements in the bath, the temperature of the water around him was not raised any higher than when he remained perfectly at rest.

BEHAVIOUR OF COLD-BLOODED ANIMALS AT FREEZING TEMPERATURE.—Doehnoff (*Reich. und du Bois' Arch.* 1872, p. 724) states that cold-blooded animals conduct themselves like plants in cold. The honey-bee dies at —1° C., the spider —3°; the flesh-fly can endure a temperature of — 6°, and the silkworm's egg survives a cold of — 21°. Part of the water contained in the leech can be frozen, and the silkworm can be solid ice without this being prejudicial to their life.

ON THE CAPACITY OF FROGS FOR RESISTING HIGH AND LOW TEMPERATURES.—Mueller (*Reichert's Arch.* 1872, p. 754) placed both Rana esculenta and temporaria in water which was then frozen. The vessel was allowed to remain in the open air five hours (temp. of air 5° and 7° R.). Afterwards the vessel was brought into a moderately warm room, and when the frog was freed from the ice, after about 1½ hours, it breathed quite lively at the surface of the water. Leuciscus rutilus treated in the same way died after a short time. When a frog has been for a long time removed from the water, so that its skin is dry, it becomes motionless, but the vital processes are not interrupted, as is shown by the circulation. Freezing completely controls the circulation at first; upon thawing, not the slightest trace of movement is to be observed in the web of the foot. Later, but slowly, the vessels begin to show signs of life. Frogs placed in water at a temperature of 20° R. were lively, at 26° they seemed tired, and at 28° death ensued rapidly. It seems probable that the temperature of warm-blooded animals produces death in the cold-blooded.

THE TEMPERATURE OF THE BODY IN HEALTH.—Juergensen, Leipzig, pp. 60, 1 plate, 1873. These papers have already appeared in the *Deutsch. Archiv f. klin. Medic.* The temperature measured in the rectum of a person at rest has a daily variation of about 1° C., the highest temp. being in the afternoon, and the lowest between midnight and morning. Taking of food slightly increases the temp. With regard to the influence of the extraction of heat on the temperature, the person experimented on found that the subjective feeling of cold during and after the bath at 9°—11° C., lasting 25′—30′, is so intense as scarcely to be endured. Bath of 30° C. and 25′ duration: during the bath the temperature measured in the rectum showed an increase. Bath of 9°—11° C. and 25′ duration: during the bath the temp. measured in the rectum showed a greater or less fall. In these cold baths, soon after the beginning of the experiment, the development of heat is diminished; it then remains some time constant, and then diminishes further. Thus, after 5 mins.' immersion in such a bath, the rectal temp. in different experiments on the same person showed a variation of 0·5°, 0·7°, 0·2° C. less than the temp. at the beginning of the experiment. Then followed a longer or shorter period when the temp. remained constant. After 25 mins.' immersion

the rectal temp. showed a diminution of 1·4°, 0·9°, 1·1° C. under the same circumstances. The after-effects of immersion in such a bath. The greatest cooling of the body takes place not in the bath, but some time (hours) thereafter. After a bath of 10°—11° C. the minimum temp. of the after-effect 33·1° (3·6° under the normal), and only after seven hours was the normal reached. The duration of the bath also influences the after-effects. Period of the day or night also. During the night the remote effects appear less than during the day. The after-effect increases with the time, and is cumulative. Thus, after a series of experiments, the after-effect was so pronounced that even after 25 mins.' immersion in 9° C. bath no diminution of the temp. was observed. Then follow chapters on the influence of quinine, muscular exercise, etc. on the temp., and also a chapter on the temp. during the first week of life.

La Chaleur Animale.—Bernard (*Revue Scientifique*, I. 133 et seq.) in the above lectures discusses the theory of Koerner and Heidenhain (*Pflüg. Arch.* IV. 558), that the difference between the temperature of the blood in the right and left ventricles is due to the proximity of the right ventricle to the abdominal organs (*Journ. Anat. and Phys.* Vol. VII. p. 358). He rejects this theory, for Hering has found that in a case of ectopia cordis the temperature of the right ventricle, even in this malformation, was higher than that of the left.

Miscellaneous.

Nucleine.—Worm Müller, as the results of his researches upon this substance, has arrived at the following conclusions (*Pflüg. Arch.* VIII. 190). Nucleine, as at present prepared, is no single characteristic chemical substance, and this is shown by the different results of different investigators as to the quantity of P. contained in it. Nucleine is probably only a mixture of organic phosphorous compounds with albuminous or albuminous-like bodies. It cannot with certainty be asserted that nucleine belongs entirely to the nuclei.

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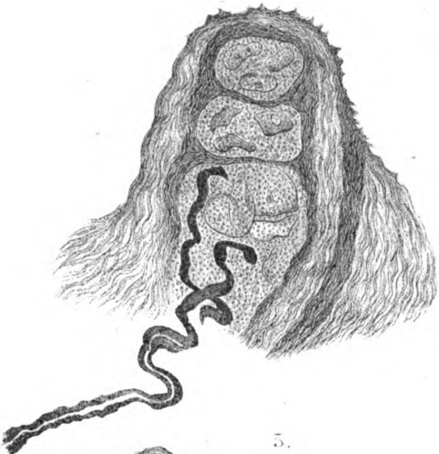

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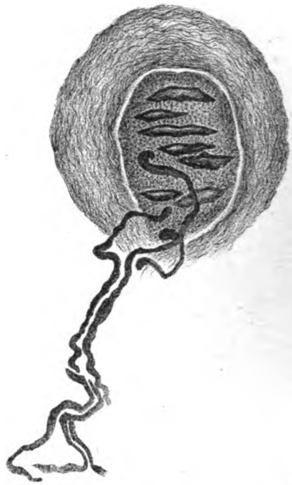

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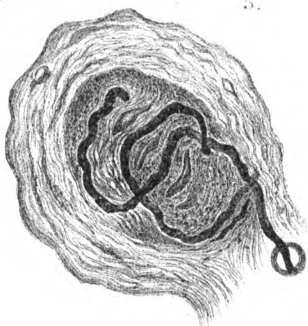

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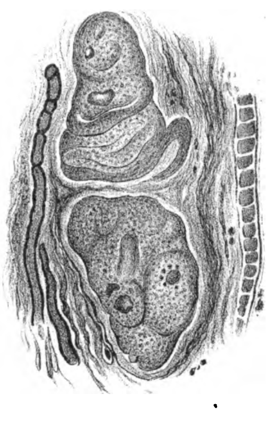

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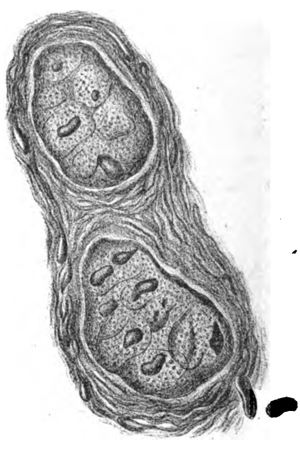

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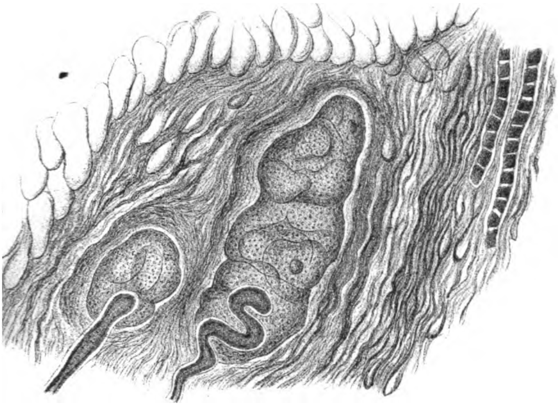

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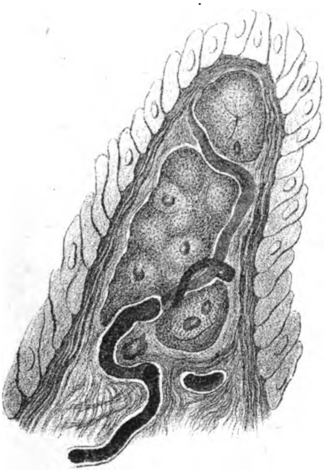

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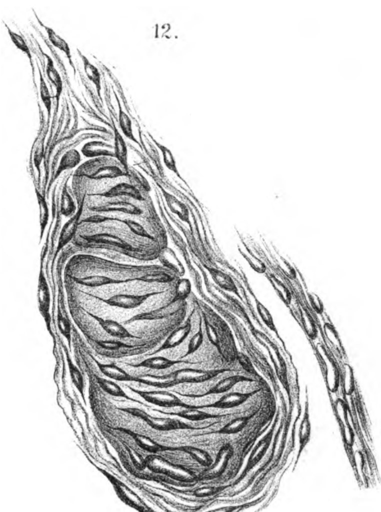

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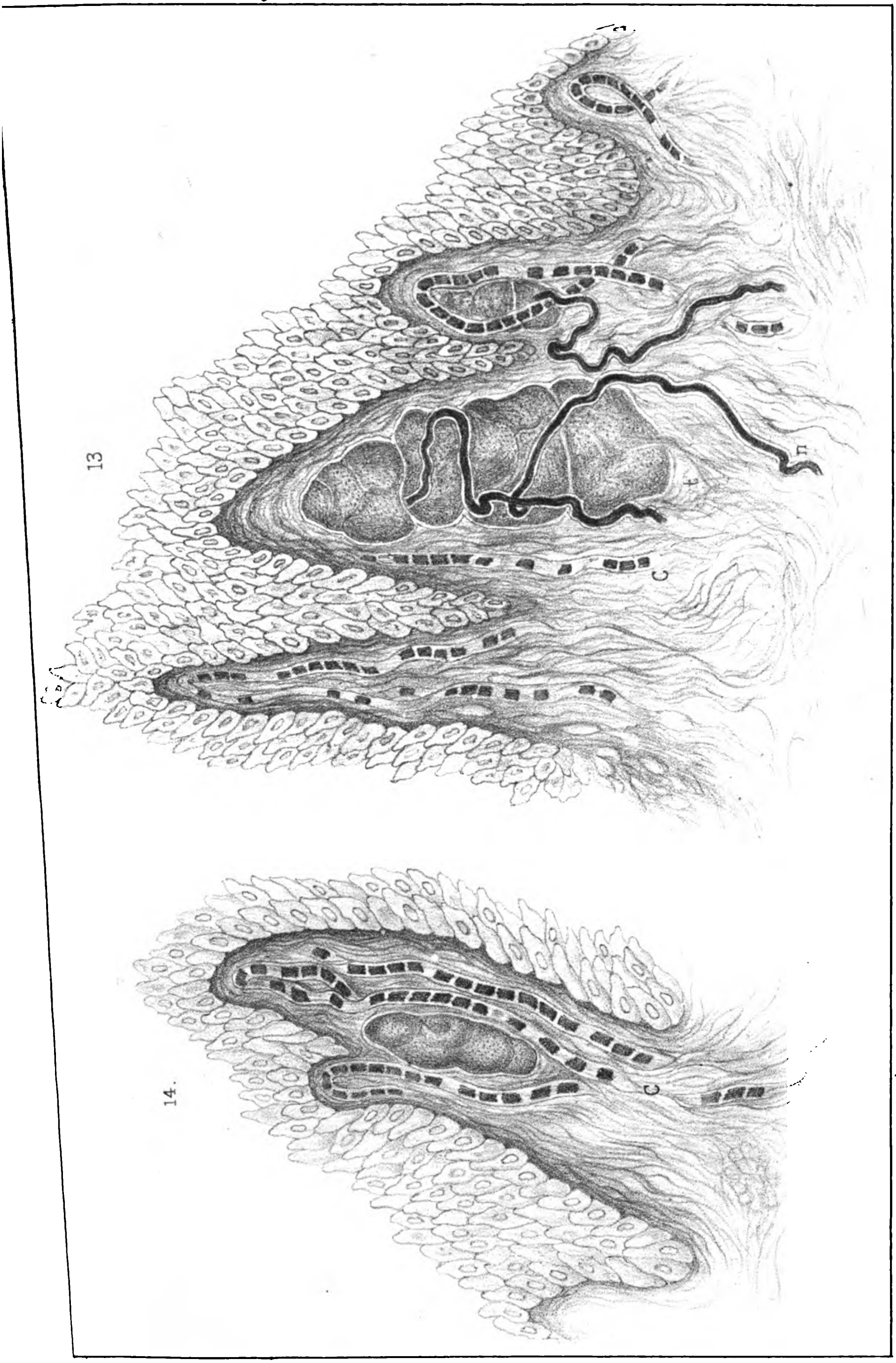

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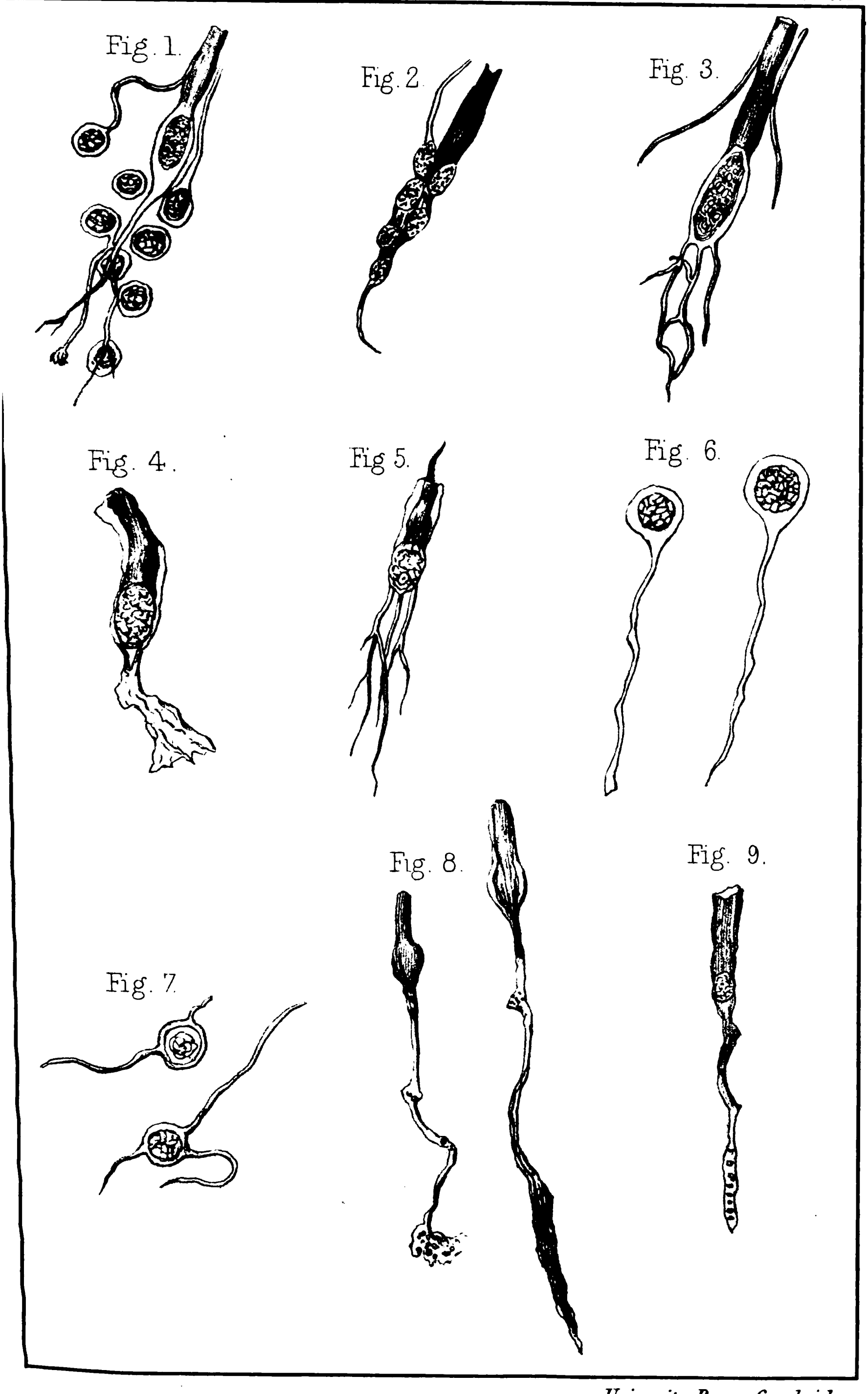

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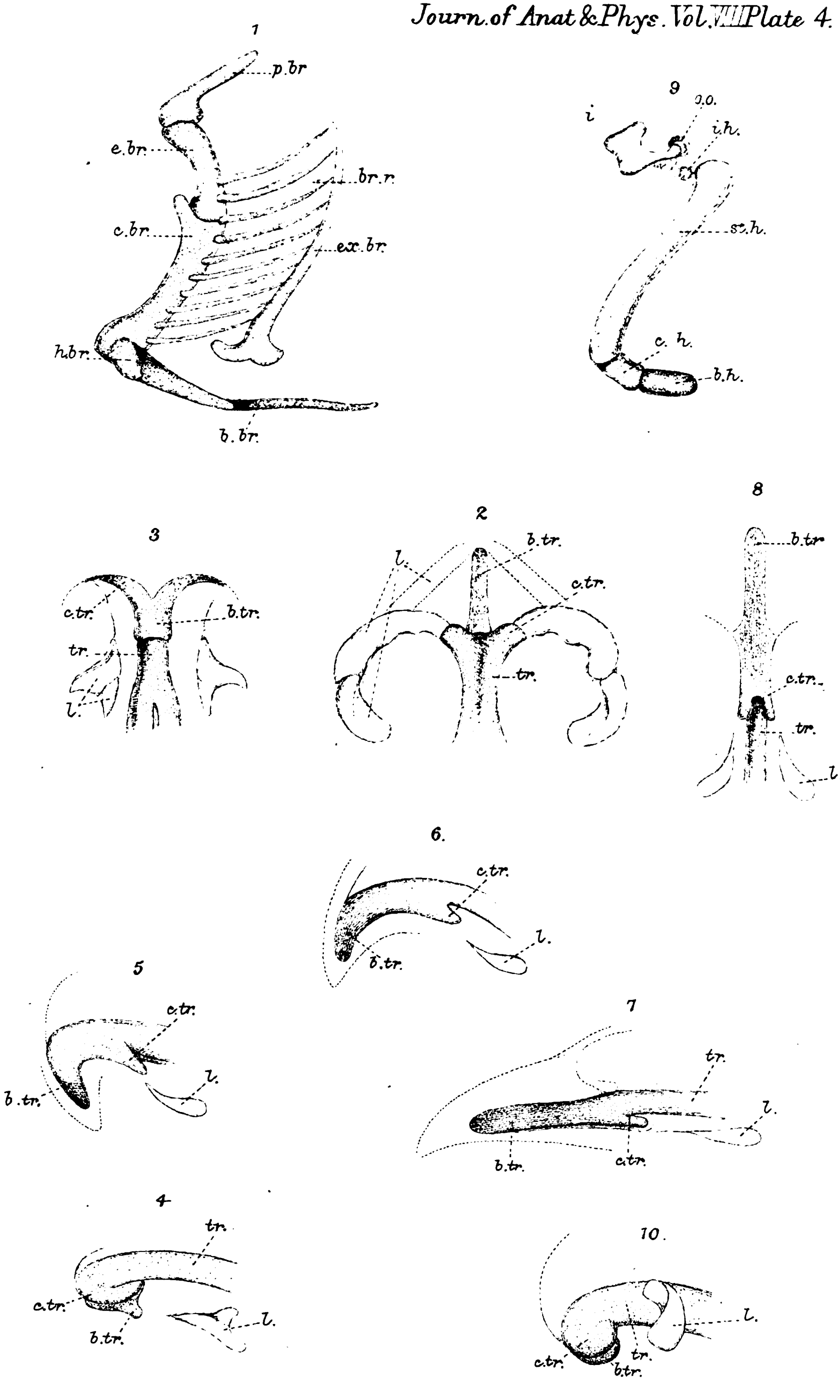

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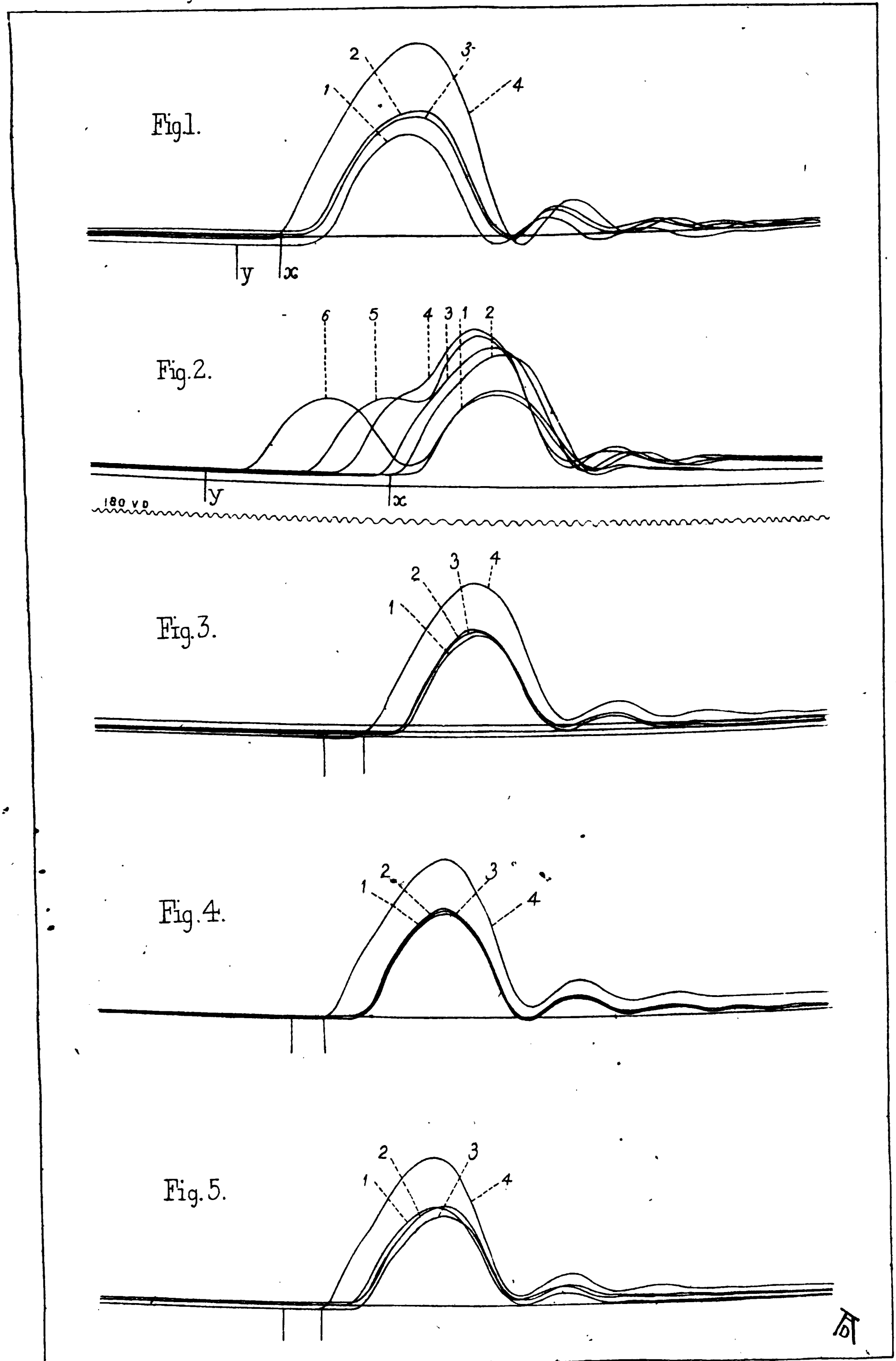

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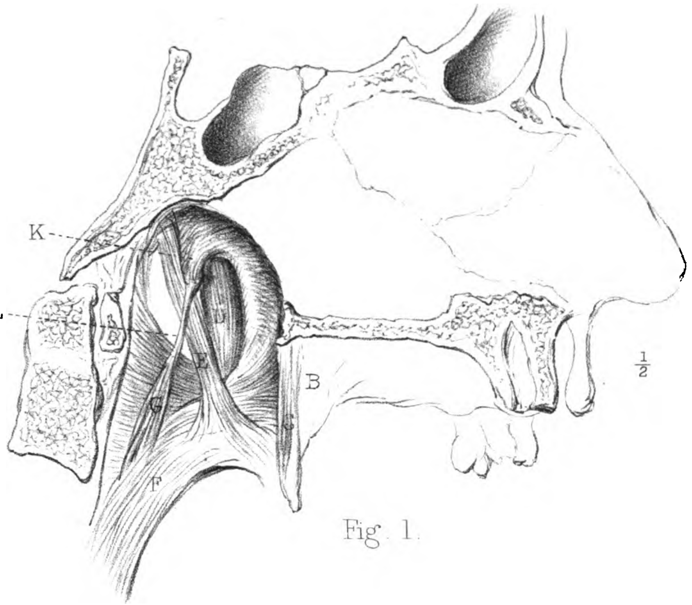

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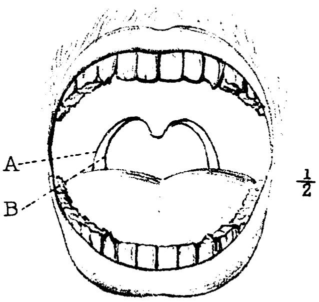

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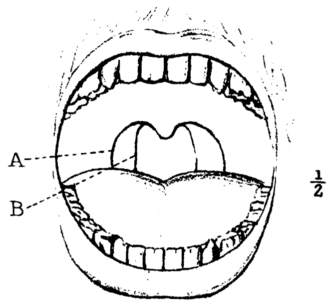

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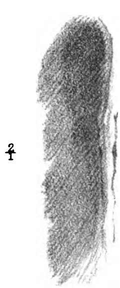

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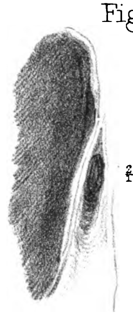

Fig. 5.

Opening and shutting of Eustachian tube.

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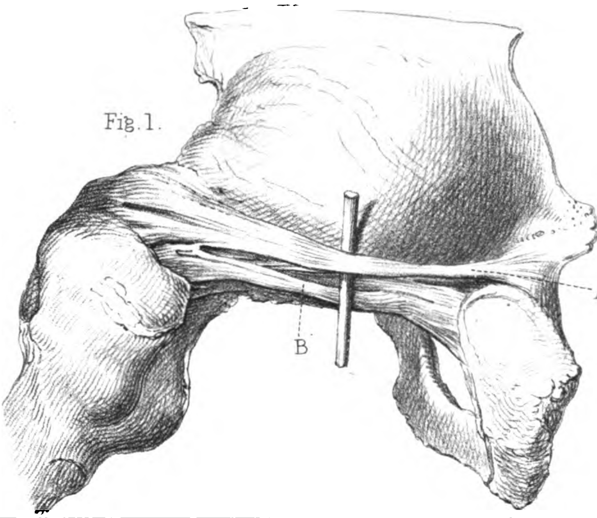

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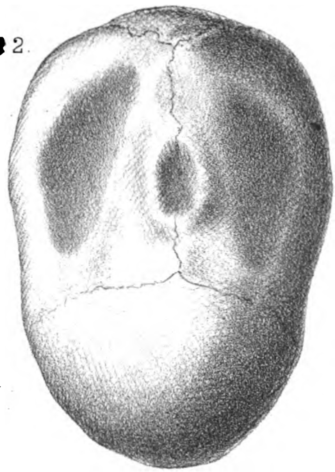

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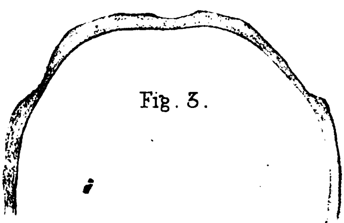

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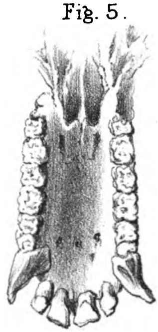

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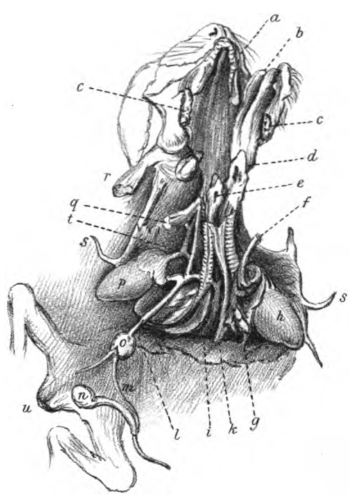

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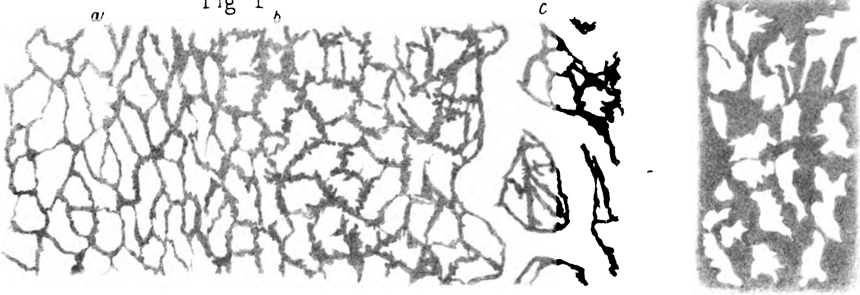

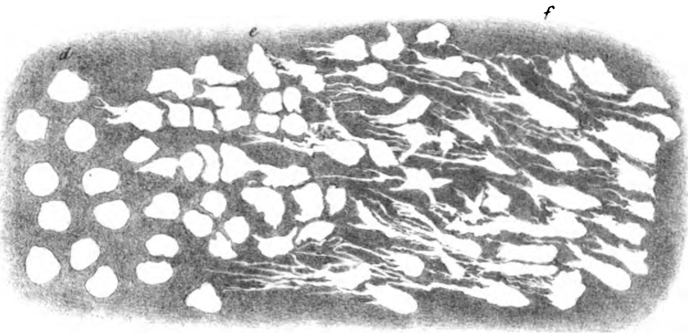

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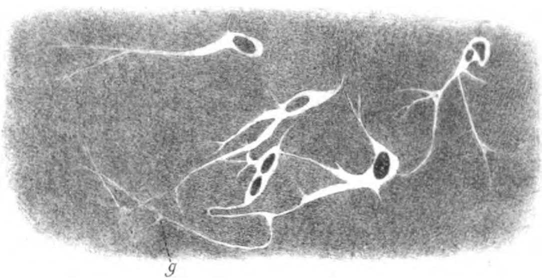

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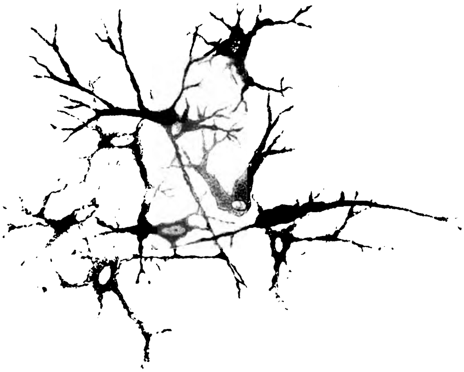

Fig. 4.

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ARTICULAR CARTILAGES AND SYNOVIAL MEMBRANES